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# CORRIGENDA

The following corrections should be made in *JHS* lxxvi, 1956, pp. 11-17:—

- P. 11, l. 8: del. *ti-ri-no*; so also l. 55 and p. 17, fn. 6  
 l. 9: del. *a-no-de-jo-si-wo*  
 ll. 29-32: for *pa<sub>2</sub>-ni-ro-we*, *de-ro-we*, *a-ra-ro-we* read:  
*pa<sub>2</sub>-sa-ro-we*, *ke-ro-we* (so also l. 55),  
*a-la-ro-we*; del. *ko-ra-o-we*  
 l. 34: del. *to-we*[, *o-ro-we*[, *a-re-ro-we*]  
 ll. 42-43: del. *a<sub>2</sub>-ro-po*, *o-wi-po-po*, *te-o-po*  
 ll. 44-45: del. all from *de-da-re-po-de* to ]*po-de* and  
 read *po-de*, *po-ru-po-de-qe*  
 l. 46: del. *i-pa*, ]*ti-pa*.  
 P. 12, l. 1: for *qo-e-to-ro* read *za-e-to-ro*  
 l. 2: for *ru-wo-to-ro* read *re-wo-to-ro*  
 ll. 57-59: for *qe-tu* read *qe-to*, del. all from "According"  
 to "improbable)" and for "would" read  
 "docs."  
 P. 15, l. 56: for the first *o-* read *wo-*.

I concede that there is virtually no evidence for or against the alternation of endings in *ti-ri-po*, *ti-ri-po-de*. In other respects my argument is hardly affected by the corrections given above.

A. J. BEATTIE

## EDITORIAL NOTE TO CORRIGENDUM TO VOL. LXXVI, p. 17 n. 7.

By a mistake in printing, the short note on Mr. Ventris' death—all that was possible in the brief interval before publication—was inserted as a footnote to Professor Beattie's article on Linear B, instead of as a separate editorial note below. This has given some the impression that Professor Beattie was responsible for its insertion and wording, and since he has been in consequence subjected to adverse criticism, I wish to state that this was not the case. Responsibility rests with the editors alone.

THE EDITOR.

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## MR. VENTRIS' DECIPHERMENT OF THE MINOAN LINEAR B SCRIPT

DOCUMENTS in the script known as 'Minoan Linear B' were unearthed at Knossos in Crete over fifty years ago. About the same time a few examples of the same script were found at Thebes and other places in mainland Greece. But it was not till 1939 that tablets like those from Knossos came to light in Mainland Greece, near the Messenian Pylos, and not till after the Second World War that they appeared at Mycenae itself.

Before the Pylian discoveries, European scholars had made several attempts to read the script; but their conclusions neither persuaded classical scholars nor wakened public interest. In this matter the discoverer of the Knossian documents, Sir Arthur Evans, seems to have imposed his own restraint on others. The acquisition of what may well be the household accounts of King Nestor created a new enthusiasm for the problem; as soon as the war ended, journals on both sides of the Atlantic began to print essays by various writers who hoped to decipher parts of the script. Mr. Michael Ventris was among these writers; and from the outset his methods were bolder and more absolute than those of the others. By 1951 his tentative decipherment of the Linear B script was being circulated privately; by 1952 he was explaining it in lectures addressed to learned societies; and a year later, in collaboration with Mr. John Chadwick, he published a full account of his solution in the *Journal of Hellenic Studies* (LXXIII (1953), pp. 84-103).

Mr. Ventris' claims are as follows: (1) The language of all the Linear B writings is Greek, and that of a pre-Dorian kind allied to classical Arcadian and Cyprian. (2) The script is in the main a syllabary, akin to the classical Cyprian syllabary. (3) By studying the way in which the syllabic signs are used (their frequency, position in the word, combination of one sign with another, etc.), and by inferring the content of the documents from certain signs which are not syllabic but ideographic, it is possible to discover the phonetic value of most of the syllabic signs. Mr. Ventris describes how he carried out the work of decipherment and produces phonetic values for most of the signs and also rules of orthography; and finally, he shows how his conclusions can be applied to various documents from Knossos and Pylos.

Mr. Ventris' theory has had extraordinary success. So far there have been few expressions of disagreement, and no journal has yet published a critical examination of the case. This is surprising enough: for the first statement of such a theory is unlikely to prove correct all along the line. It would seem natural that after the decipherment had been applied to a wider range of documents, a number of details should need modification. In fact, however, the original phonetic values published in the 1953 article are still virtually unchanged; and, in general, the amount of Minoan Greek that can be read and understood is still substantially the same as that which was announced in the first instance. This is the crux of the matter: a few documents can be interpreted, but a great many are incomprehensible. This situation inevitably suggests not merely flaws in detail but some degree of fundamental error. And therefore it is necessary to test Mr. Ventris' theory thoroughly.

I need not describe the Linear B documents in detail here: a full account, with illustrations, is to be found in Mr. Ventris' article. Most of the texts are written on small tablets of unbaked clay. The writer used a fine stylus; he first ruled a series of parallel lines on the wet surface of the clay, and then he wrote from left to right between the lines and from top to bottom of the tablet. Short groups of signs, evidently words, were divided from each other by a short vertical stroke. Most, or all, of the tablets seem to contain lists of people, animals, goods, and the like. The writing consists partly of words, which sometimes occur singly or in short sequences, and sometimes in longer sequences, amounting perhaps to continuous prose. But it consists also of ideograms, representing commodities, quantities, values, etc., and of numerals. Wherever Linear B script is found, the signs are virtually the same in number and form, and words, both singly and in groups, recur in one place and another. Hence it seems that there is one language in use in all localities. Conversely, there is no indication of a second language in any locality.

So far all is agreed. And when Mr. Ventris proceeds to argue, on historical and archaeological grounds, that the language of the tablets is Greek, it is impossible to refute him. Many scholars in the past have held this view, and many still hold it. I, too, am ready to admit that the language is as likely to be Greek as anything else, although I maintain that there are other possibilities. The question that concerns me now is whether, given that the tablets may be written in Greek, Mr. Ventris' decipherment is correct.

Arguing from the number of different signs used to form words (80-90) and from the nature of oriental scripts roughly contemporary with Linear B and also from the later Cyprian script, Mr. Ventris claims that Linear B is a syllabary and that each syllabic sign is of the type *TA* (in which *T*



represents any consonant and *A* any vowel) or of the type *A* (representing any vowel).<sup>1</sup> This, too, we may accept as entirely plausible.

We are now on the threshold of decipherment. Mr. Ventris says that he divided the signs into categories—first of all, those that were in common use, or were used rarely, or were of average frequency; secondly, as they were found at the beginning of words or at the end; and thirdly, as they appeared to occur in special combinations or to alternate with each other at the end of words and in other positions. These calculations suggested to him, amongst other things, the distinction between pure vowels and other signs; for the signs for pure vowels should occur mostly at the beginning of words and rarely in the middle, except as the second part of a diphthong. The figures also suggested certain features of the Greek inflectional system, including number, gender, and case; for many words seemed to show several variations of the final sign in different contexts. Thus early, even although no sign had been given a precise sound-value, the grammatical framework of the Greek language began to show itself.

Apart from the question of frequencies and alternations, Mr. Ventris also set himself to observe how a particular word, or even a group of words, recurred (often with modifications) alongside a particular ideogram or numeral. From these observations he tried to infer the nature of the context and so to determine whether certain words might represent personal names, place-names, occupations, or the like. On the other hand, when he found that a given word recurred again and again in contexts seemingly unconnected with each other, he decided that it probably belonged to a more general kind of vocabulary.

The next part of Mr. Ventris' work was of a complex nature. He tells us that he first constructed a grid or table, on which he arranged the syllabic signs lengthwise and crosswise according to the alternations which he had already observed, and that, secondly, he proceeded to examine particular words and word-endings which seemed to offer some scope for conjecture. So, by trial and error, giving experimental values to certain signs and testing each of them against the grid, he gradually identified a number of the signs. That is to say if he thought that a word might contain the suffix *-oio* or that it might correspond to Grk. *πατήρ*, he would apply the values *-yo*, *pa-*, *te-*, etc., to certain signs; and he would try to find out whether these values made plausible words or parts of words in other contexts; and he would control the whole operation by ensuring that the frequency of the signs concerned under varying conditions corresponded to that of the syllable concerned in later Greek.

Now all these experiments involved a high degree of conjecture—much higher than Mr. Ventris seems to have realised. And we have not yet considered all the factors: for, even while he was assigning sound-values to the signs, Mr. Ventris was also formulating the rules of orthography, which would not only determine the interpretation of the documents in general but would also pinpoint variant spellings, variant word-forms, and phonetic trends. This, too, involved much guess-work and many arbitrary decisions. For the present, however, I leave this important matter and consider first the use of the grid.

In his article Mr. Ventris shows us a table of comparisons, consisting of pairs and series of words that are identical in all syllables but one, or nearly so identical. Unfortunately this table is printed in Roman script, not in the original signs of Linear B, and the sounds are already divided into *Vowels* and *Consonants* and fully classified. We are not given the original grid, without sound-values, and we are not shown all the stages of identification. Mr. Ventris assures us that the grid was completed before the equivalents were added, but he does not give us the figures from which he made the table. He does not say how often *ke* or *pa* occur in various positions, or how often each alternates with *ki*, *ka*, *ko* or *pe*, *pi*, *po*, or how often each occurs before or after other signs. Let us suppose that he used *all* the texts available to him, and that he counted every single sign in initial, medial and final positions, and so obtained three figures and an overall total for each sign, as well as an assortment of information about alternatives or concomitants. Are we then to suppose that these figures fell naturally into groups, so that the signs to which they referred could be disposed lengthwise and crosswise in such a way that they would ultimately be found to correspond to series of the type, *i*, *pi*, *ti*, *ki*, etc. and *pa*, *pe*, *pi*, etc.? This is evidently what Mr. Ventris means us to believe. Yet, if we bear in mind that the tablets are relatively few in number and in many cases fragmentary, that they deal with a very small range of subjects, that the words in them are often tabulated in lists, and not used in ordinary syntactical combinations, and that some words are repeated again and again in identical or similar contexts, it appears very unlikely that either the words or the syllables in them could give a fair representation of the language to which they belong. On the other hand, if we consider that mere enumeration of *syllables* (as opposed to individual speech-sounds) must give a flat, undifferentiated image of the sound-pattern of any language, we must infer that Mr. Ventris has taken considerable risks in constructing his table. Most striking of all, however, is Mr. Ventris' assumption that, if two words begin with the same succession of signs

<sup>1</sup> The symbols *T* and *A* will be used from time to time in the argument that follows to represent, respectively, any consonant and any vowel. They may also be used in conjunction

with letters; e.g. *Ta* = a syllable consisting of any consonant plus the vowel *a*.



(two, three, or more in number) and differ only in the last sign, these two words are necessarily or even probably connected with each other in structure and sense. In the case of short words, especially, such an assumption is wholly unjustified; the resemblances may not extend beyond the mere sounds and in other respects may be accidental. Consider in this light πρόπος, πορθέω, πορθμός, πόρτις; πότερος, ποτήριον, ποτάομαι; τρίβω, τρισί, τριχός; etc. But again and again, it seems, Mr. Ventris put two words together in this way and then proceeded to identify the final syllables of each on the hypothesis that one word was a by-form of the other. We should be content to accept both his hypotheses and his conclusions if his table were found when complete to consist exclusively of Greek words containing prefixes, roots, suffices, and endings that alternated in Greek style. A brief scrutiny of the table shows that this is not the case; many of Mr. Ventris' comparisons lie in the realm of conjecture—as much now, after decipherment is complete, as at the outset.

Consequently I regard the table of comparisons and the grid with strong suspicion. But now I pass to the evaluation of the signs.

Mr. Ventris noted first of all five signs which occurred more commonly at the beginning of words than elsewhere and identified them collectively with the five pure vowels of Greek. Next he observed that the rarest of these five vowel-signs sometimes occurred also at the end of words. When this sign was final, it was, on the one hand, preceded by any of thirteen other signs, which were all of about the same frequency, and on the other hand it was sometimes replaced by a particular one of these thirteen signs. His interpretation of these facts is as follows. The vowel-sign which occurs initially and finally is *u*. The thirteen signs which precede *-u* when it is final form the series *Te*. And the particular sign that sometimes replaces *-u* in these words is *-we*. Now, if it is assumed that final *-s* is never written, the conclusion is that all the words concerned belong to the Greek *-ηf-* declension, showing nom. sing. *-εύς* and plural *-ήφες*. Furthermore, since these same words sometimes appear with yet another final sign, this sign can be interpreted as a further case-form of the declension, to wit *-wo*; thus we identify the genitive *-ήφως* or *-ήφων*. For Mr. Ventris decides that not only final *-ς* but final *-v*, and also *-ρ* and post vocalic *-ι*, should be left out.

Thus by a single experiment, Mr. Ventris settled the value of the sign for *-u*, the entire series of thirteen signs for *Te* (though not of individual signs within that series, except *we*), and also one member of the series *To*. His grid then indicated to him all the other members of this *o-* series, and all that remained under this head was to identify each sign in the series through trial and error.

This procedure is so bold that we must consider it with some care. I have described it much as Mr. Ventris himself describes it, although more simply and more briefly. But it can be put more simply still. What Mr. Ventris is dealing with here is a list of words, each of which admits three variant signs at the end. One of these three variants is a sign that is otherwise rare at the beginning of words. Another of the three is one of a group of thirteen signs that form the penultimate sign in all the words. On this evidence, and nothing more, Mr. Ventris identifies the *-εύς* declension. I shall consider the validity of this identification later on; for the present it is enough to note the extreme paucity of the evidence. For Mr. Ventris does not say how many words the list contains, or how often each word occurs, or what circumstances suggested to him that these words should be *-εύς* nouns rather than something else.

In the meantime I continue to follow his account of the decipherment. Having found *-wo*, Mr. Ventris fixed his attention on another sign in the same column of the grid. This sign was often final, and it occurred at the end of words which the context suggested might be in genitive relationship to neighbouring words. So Mr. Ventris decided that it signified *-yo*, and that it often represented the last element of the genitive ending *-oto*. Sometimes, however, this same sign belonged to the *-yo* suffix of adjectives (final *-ς*, *-v*, and *-ι* being omitted, according to rule), and thus, by further consideration of the grid, it became possible to pick out *-ya*, the feminine counterpart of adjectival *-yo*. From this point Mr. Ventris moved on, guided by various contexts, to identify adjectives ending in *-ios* (*-i-yo*) and so to pick out the whole series *Ti* and to fix the pure vowel *i* itself. And so on. Before long he had recognised each of the five pure vowels and every sign in the series *wa* and *ya*. All the other signs had been classified according to their vocalic element, and it remained only to fix precisely the consonantal value of each.

This final phase involved a good deal of experimentation. For example, Mr. Ventris conjectured that a group of tablets bearing ideograms which are thought to represent severally 'men', 'women', 'children' contained a reference to the concepts 'father' and 'mother'. He then took two words, each of two signs, that recurred in these tablets and assigned to them the value *pa-te(r)*, *ma-te(r)*. These identifications started the process of delimiting the *p*, *m*, and *t* series of consonants. Again, two categories often mentioned in Cretan tablets were guessed to be boys and girls, represented by *ko-wo*, *ko-wa*, i.e. *κόρφοι*, *κόρραι*; and two terms which recurred in connection with these categories were identified as *me-zo*, *me-u-jo*, i.e. *μέζων*, *μέλων*. And thus the *m* series was carried a stage farther. Yet again, the total figure at the end of a list of numbered commodities sometimes has alongside it a word that might signify 'total'. This word, with its variable ending,



was interpreted as *to-so-i(-de)*, *to-sa(-de)*; and so the *t* series received another item, and the *s* and *d* series began to come into line.

By this time there were few common signs that did not have a phonetic value set alongside them. In his *JHS* article Mr. Ventris was able to offer an interpretation of no less than sixty-five syllabic signs.

As the phonetic evaluation of the signs advanced, the rules of orthography became more definite. At an early stage, however, the decipherer found himself compelled to allow for variant spellings and even for misspellings and grammatical faults. He postulates the existence of two signs of roughly the same value ( $a_1$ ,  $a_2$ ), or the occasional omission of a *y*, *w* glide, or even a grammatical mistake. In some of these matters Mr. Ventris plainly goes too far; it is impossible to correct a Mycenaean writer's spelling, still less his grammar, before you are sure of the rules which he observed and also of the phonetic value of his script.

I mention only the more important of Mr. Ventris' spelling laws. They are as follows:

(a) The five vowels written are *a*, *e*, *i*, *o*, *u*. No distinction is made between long and short vowels, or between long and short diphthongs.

(b) Diphthongs with *-u* are shown; diphthongs with *-i* are not normally shown—except initial *ai-*, and *ei*, *oi*, *ai* before final *-s*. Intervocalic *i* is shown by the series *yA*.

(c) The continuant consonants are *m*, *n*, *r*, *s*, and *z*. Greek *l* is shown as *r*. The continuants *m*, *n*, *r*, *s* are omitted at the end of a syllable, whether medial or final.

(d) The stop consonants are *p*, *t*, *k*, labiovelar *q*, and *d*. Of these *p*, *k*, *q* are voiceless, voiced, or aspirate; *t* is voiceless or aspirate; *d* is the only representation of a voiced stop sound. There is no doubling of consonants.

(e) Consonantal groups of *stop + stop*, *stop + continuant* are indicated by two signs, each of which has the vowel quality of the vowel that follows the group.

(f) When *s + stop* occurs initially, the *s* is omitted.

Armed with his table of phonetic values and with his rules of orthography, Mr. Ventris devotes the rest of his article to the discussion of various types of document, grammatical categories, syntactical combinations, dialectal peculiarities, and so forth. Here are some of his results.

Tablets from Gnosso contain the words *Ko-no-so*, *Ko-no-si-jo*, *Ko-no-si-ja* and also *A-mi-ni-si-jo*, *A-mi-ni-si-ja*, and Pylian tablets often have *Pu-ro* at the top. This is at first sight encouraging. But of course we do not know whether Mr. Ventris used these particular words in the first instance to establish the value of one sign or another. Consequently it is uncertain whether his transcription of any word confirms the identification of any or all of the signs in that word or is merely the first guess by which one or all of these signs were identified. At any rate, no tablet has yet appeared in which the interpretation of these words as place-names is confirmed beyond reasonable doubt by the context.

We go farther and find phrases that make sense within the prescribed rules and are in harmony with an ideogram. For example, if *to-sa pa-ka-na* is followed by a sign representing a sword with a numeral beside it, then *τόσσα φάσγανα* seems a reasonable transcription. Nevertheless, the rules of orthography admit other interpretations, and one wonders why both the word for 'sword' and the ideogram appear. The coincidence is the more impressive if the phrase is longer. Thus *Pu-ro i-je-re-ja do-e-ra e-ne-ka ku-ru-so-jo i-je-ro-jo* followed by the ideogram for woman and a figure 13 may be rendered Πύλος: ἱερείας δοέλαι ἔνεκα χρυσοῖο ἱεροῖο. WOMEN 13. On reflection it may be seen that ἔνεκα is in the wrong place and bears an unusual sense,<sup>2</sup> that the form *do-e-ra*, δοέλαι is unsubstantiated, that *i-je-re-ja* is a false form for *i-je-re-wi-ja*, and that the phrase as a whole has no meaning that is both obvious and plausible. In relation to the Mycenaean civilisation, the sale of slave-women for 'sacred gold' is pure fantasy. Yet the general aspect of the transliteration is undoubtedly Hellenic; and if all else were confirmed, we might not raise any strong objection to this particular case.

Again, when we find a text consisting of (line 1) *a-la-na-po-ti-mi-ja-* (line 2) *e-nu-wa-ri-jo pa-ja-wo po-se-da*], we may well feel a thrill of excitement on recognising Ἀθάνᾱ Πότνια Ἐνυφάλιος Παιάφων Ποσειδά[ρων. The thrill vanishes, however, and disappointment succeeds it, when we consider that this is a most implausible quartet of gods; that Ἀθάνᾱ Πότνια could hardly be written as one word and would almost certainly appear in the order Πότνια Ἀθάνᾱ; that Enyalios as a cult-name is not likely to be earlier than Homer, for whom it is a conventional epithet; that Paian is properly an invocation and only by poetic artifice a name; and that *po-se-da* is an incomplete word—the only comparable word in Linear B is *po-se-da-o* and not *po-se-da-wo*. The words on the tablet are only the left-hand portion of a text which one would expect to be, like its fellows, an account of men and goods. It would be more than venturesome to base a theory on this fragment.

<sup>2</sup> Mr. Ventris uses the obelisk-sign to mark 'suggested spellings, meanings, and compounds which are not paralleled in classical Greek, or implicit in the accepted etymology'.

He uses a question-mark 'where serious difficulties stand in the way of the meaning or spelling proposed'.



It is indeed a difficulty that the intelligibility of the documents seems to diminish as their length increases. Here is one of several longer texts quoted by Mr. Ventris:

<i>Pa-ki-ja-ni-ja to-sa da-ma-te</i>	DA 40
<i>to-so-de te-re-la e-ne-e-si</i>	MEN 14
<i>wa-na-ta-jo-jo ko-to-na ki-ti-me-na to-so-de pe-mo</i>	GRAIN $2\frac{1}{60}$
<i>o-da-a<sub>2</sub> o-na-te-re e-ko-si wa-na-ta-jo-jo ko-to-na</i>	
<i>a-tu-ko e-te-do-mo wa-na-ka-te-ro o-na-to e-ke de pe-mo</i>	GRAIN $\frac{1}{60}$

He offers the following explanation:

Σφαγιάνια τόσα †Δαμάτηρ	DA 40
τοσσο(δε) †τελεσται ἐνέενσι (= ἐνεῖσι)	MEN 14
φαρναταίοιο κτοίνας κτίμεναι τοσσόνδε σπέρμο	GRAIN $2\frac{1}{60}$
‡= αὐτὰρ †ὄνατῆρες ἔχονσι φαρναταίοιο κτοίνας	
*Ατυχος †έντεσδόμος †φανάκτερος †ὄνατον ἔχει [τοσσόν]δε σπέρμο	GRAIN $\frac{1}{60}$

Here, among other peculiarities, σπέρμο with final -ο is supposed to be a sign of Arcadian affinities (in token of which *e-ne-e-si*, *e-ko-si* are transliterated ἐνέενσι, ἔχονσι, despite the fact that *n* is not written). But it is doubtful whether even in fourth-century Arcadian neut. -μα would appear as -μο, and in Mycenaean times it is most improbable. Now this sentence, if it is a sentence, has an agricultural flavour throughout; and that is something. But anyone acquainted with the Greek tongue can see that this is not a Greek text. Α κτοίνα is not a farm or field, as Mr. Ventris thinks, but a village community (Hesychius' explanation, δῆμος μεμερισμένος, refers to the division of a township into smaller units, i.e. villages); and κτίμενος would not, as Mr. Ventris thinks, mean 'established', nor even 'cultivated', but merely 'built', 'constructed'. It is unnecessary to remark that ὄνατῆρες, έντεσδόμος, and φανάκτερος are fictitious or that ἐνέενσι is morphologically unacceptable. The whole thing is nonsense from beginning to end.

The farther I follow Mr. Ventris, the more I fear that he has led us off the track. From time to time he tries to reassure me; he says that if we are not on the right road we cannot be far from it, or that we may have diverged slightly but may soon be back on the road, or even that the road is none the worse for not being exactly what we expected it to be. The Greek, he says, departs from the conventions of classical Greek because it is five hundred years earlier than Homer, because it is a dialect with which we are not familiar, because a few of the syllabic signs may have been wrongly evaluated, and so forth. These excuses, by their multiplicity and constant repetition, intensify my doubts. Therefore I go back to the starting-point and explore the road again.

I consider first the general pattern of Mr. Ventris' syllabary. There is nothing improbable in the notion of Greek being written with 80-90 syllables of the type TA. The Cyprian script in the classical age shows it is possible to write Greek using fewer syllables and without distinguishing the length of vowels or even the length, voice, and aspiration of consonants. The Greek alphabet itself, although it is fairly exact about consonants, did not show the length of every vowel; and in the beginning it did not distinguish vowel-length at all.

Nevertheless, there must be a limit to the number of phonemic differences that can be left out. Otherwise the script will become too inexact to be of any use. So in Greek, if you do not show separately the five cardinal vowels and both the -i- and -v- series of diphthongs, you run the gravest risk of being misunderstood. If you do not write -v and -ς and -ι, you destroy the syntax of your sentences. If you confuse ρ and λ, you obliterate the distinction between important suffixes and you obscure many roots. If in addition to all these things you omit a variety of medial consonants, you create havoc.

Mr. Ventris' syllabic pattern is really far too simple, and we may say with confidence that it is insufficient for the writing and reading of Greek. It irons out the sound-system of the language. On the other hand, just because it is so imprecise, it enables Mr. Ventris to discern Greek words in groups of syllables that look entirely un-Greek to the classical scholar.

Another fault of Mr. Ventris' syllabary is that it is unsymmetrical. Nature imposes a degree of phonetic symmetry on all languages, and so also on the scripts that are used to represent them. For example, people who distinguish *k* from *g* in writing will generally find it necessary to distinguish *p* from *b*, and any other unvoiced sound they use from its voiced counterpart. Mr. Ventris imagines that the Mycenaeans habitually marked -u- diphthongs, yet wrote the no less important and far commoner -i- series as if they were not to be distinguished from the cardinal vowels. But occasionally they would depart from this rule by writing *ai-* initially (though not *Tai-*) and by adding *i* to the cardinal vowel when the *ai-* or *oi-* diphthong preceded final -s. Such inconsistency borders on the incredible. Again, Mr. Ventris holds that *p*, *k*, *k<sup>w</sup>* were not distinguished from *b*, *g*, *g<sup>w</sup>* or from *ph*, *kh*, *kh<sup>w</sup>*, but that *t* and *d*, *s* and *z* were regularly kept apart. This, too, surpasses belief. Such features are not to be excused on the ground that the Greeks took over a syllabary which had been



designed to represent another language. It is not likely that any other language would have had a sound-system as impoverished or as lop-sided as the system which Mr. Ventris postulates.

From comparative philology we know something of the sound-system of Greek in the Mycenaean age or thereabout. It bears little relation to Mr. Ventris' decipherment. One of the most surprising features of the decipherment is its *complete* labio-velar series, *k<sup>w</sup>A*. It is possible, of course, that at this time the IE labio-velar series was still recognisable as such in Greek and was not yet divided into dentals, labials, and gutturals. But it is very unlikely that it was clearly distinguished in all circumstances from these other sounds. Another fact of prime importance which Mr. Ventris neglects altogether is the prevalence of the glottal spirant *h* in Mycenaean Greek. This sound, arising from *y*, *s*, and combinations containing these sounds, must have been very common both initially and medially; and it must have been plainly articulated—if indeed the original *y*, *s*, etc., were not still to some extent preserved. Even if it was not thought necessary to distinguish aspirated from unaspirated stops, there must have been a pressing need to mark the presence or absence of *h*. Writers who had not the means of doing this at the outset, would have had to create signs for the purpose. A further difficulty is that Mr. Ventris gives us simple *s* or *z* where classical Greek has *s*, *z* or *ss*, *t*, *tt*, *d*, *dd*, these variants being the outcome in the main of pre-historic groupings of *t*, *d*, *th*, *k*, *g*, *kh*, with *y*, *w*, *s*. There is no reason whatever to suppose that these groupings had already in Mycenaean times reached a stage indistinguishable from that in which they are found in classical Ionic. We should expect either a special series of syllables to represent these complex sounds, and in particular something like *ts*, or, alternatively, if the sibilant and dental series *were* used, violent fluctuations in the spelling.

To sum up, Mr. Ventris' version of Linear B is inadequate for writing Greek; it lacks the symmetry natural both to speech-sounds and to the conventions of writing; and it does not represent the outstanding characteristics of Greek pronunciation in Mycenaean times.

Similar defects can be found without difficulty in the rules of orthography, which depend closely on the pattern of spoken syllables and syllabic signs. Since the number of signs is severely limited, each of them is given a wide variety of functions. For example, *ka* may represent *kā*, *kā*, *kāi*, *kāi*, *kās*, *kās*, *kār*, *kār*, *kāl*, *kāl*, *kām*, *kām*, *kān*, *kān*; *gā*, etc.; *khā*, etc.; *skā*, etc.; *skhā*, etc. This one sign may have no fewer than seventy values, all of which, except the variable length of the vowel, would be rigorously distinguished in classical Greek. Not all Mr. Ventris' syllables admit as many variants as this; but it is easy to see that on his assumptions a group of three, four, or five syllables may admit several hundred or even several thousand possible readings, and a series of half a dozen words runs easily into thousands, hundreds of thousands, or even millions. This consideration is not enough to rule out the possibility of Mr. Ventris' hypothesis being correct. For usually anyone who has to read at all is given a clearly defined and easily recognised context and can automatically rule out many false and irrelevant interpretations that might otherwise occur to him. Besides, a proportion of the variants when combined with others would be rejected instinctively as un-Greek. Even so, the fact remains that the multiplicity of interpretations possible in Mr. Ventris' scheme is so overwhelming as to be quite unacceptable.

The rule on which this decipherment chiefly depends is that which obliterates the second consonant of every closed syllable ending with *m*, *n*, *r* (*l*), *s*, *y*. I.e. *TAT*, whether medial or final in the word, is written as *TA*, so that *χαλ-κός* becomes *ka-ko*, and *κα-νός* would be *ka-no*. (The omission of initial *s* before a stop and of initial *w* before a continuous consonant also comes under this rule. Here the scheme is consistent; but that is hardly a sufficient defence.) The absence of so many medial consonants confuses many roots and suffixes, and the absence of final consonants destroys most of the Greek word-endings. This defect, combined with the multiple values of the consonantal signs that are shown, makes the interpretation of Mr. Ventris' transcriptions a fascinating pastime. For example, if you should despair of *Δαμάτρη* in the long text I have quoted (p. 5), you may try *δάμαρες* 'wives', or *δάματα* (du.) 'portions' (Hesychius), and reinterpret everything afresh.

Under such conditions it is ridiculous to talk of orthography at all. Greek cannot be written in this way; or, if it were, it could not be read. To make Greek intelligible in any script word-stems, suffixes, and inflected endings must be represented adequately. If any one of these three elements does not emerge clearly, there is uncertainty; if two or all three of them are left obscure, as is often the case with Mr. Ventris' decipherment, words and phrases become completely incomprehensible. There can be no appeal in this matter to scripts which are used to represent languages of a different structure from Greek. In Semitic languages it is often possible to form a short-hand system by writing the consonants and omitting the vowels. But this is because the consonants in these languages contain the essential meaning, and vowels serve mainly to define the function of the word in the context; so that if you know the word divisions and the word order you can without too much difficulty construct the context and fill in the vowels. You must, however, have *all* the consonants. In his Mycenaean Greek Mr. Ventris would have us make do with a feeble selection of vowels and consonants, upon which he imposes any interpretation that suits his fancy.

In documents which purport to be a record of official accounts, this kind of spelling, is of course, particularly unsatisfactory. No one would know whether a scribe who wrote *e-ke* meant *ἐκε*,



είχε, or ἔσχε (if indeed he did not have ἔκει, εἶ κε, ἔλκει, or something else in mind). *Pylia*s, who were concerned either with *tribes* or *wheat*, would hardly know what to make of *pu-ro*; they might in the end come to the conclusion that it meant an *ankle* (σφυρόν) or (since we are allowed sometimes to count *o* as *a* in Arcadian style) a *hammer* (σφυρα).

An obvious objection to this part of Mr. Ventris' case is that many of the ambiguities need not arise if only he allowed the Greeks to make full use of their script. Anyone who wanted to write *κακός* and *χαλός* according to Mr. Ventris' decipherment would naturally put down, not *ka-ko* twice, but *ka-ko-se* and *ka-ro-ko-se*. This would not be perfect, but it would be a few degrees better than *ka-ko* for both words. Here Mr. Ventris cannot argue that the Greeks inherited a barbarian script, and, with it, the rules of orthography devised for a barbarian tongue. If that had been the case, someone—whether genius or simpleton, or merely a man of good sense—would surely have improved on *ka-ko* and the like. The temptation to write something to indicate an -λ- or an -ς must have proved irresistible.

I pass briefly over minor points of spelling that appear illogical and improbable. For example, we are told that *sk*, etc., are written simply as *k*, etc., but that *ks*, *ps* are shown in full. Although there is a rule that *κτε*, etc., are shown as *ke-te*, etc., we come across striking exceptions. For example, *wa-na-ka-te-ro* is made into *φανάκτερος*, a purely fictitious word. *E-wi-ri-po* is read as *Εὔριπος*, although initial *e-u-* is common and might well have been used here. *Ru-ki-to* is alleged to mean *Λύκτος*, on the analogy of *ru-ki-ti-jo*, *Λύκτιος*; but the analogy would be more likely to work the other way. These things are really unacceptable, and they are serious enough in themselves. They are, however, subordinate to the general criticisms which I have already set forth. The essence of the matter is that Mr. Ventris' rules of spelling have one advantage and only one; they allow him to make something like Greek out of many successions of syllables that would otherwise be thought barbarous. Against this, they have the serious disadvantage that neither Mr. Ventris nor anyone else can ever be sure what is in fact meant.

Mr. Ventris' decipherment started from the identification of suffixes. Provided that the language is Greek or akin to Greek, it would seem reasonable to begin with suffixes. If the first identifications were right, all might be well (but need not be so). If the first few were wrong, the rest would certainly go astray. At all events, if the commonest final syllables were equated with Greek suffixes, some of the texts must look like Greek. And in a three- or four-syllable word, if the last one or two syllables are read as a Greek suffix, there is a fair chance that the one, two, or three syllables before them may correspond to a Greek word-stem or resemble a Greek word-stem. The main question is, whether Mr. Ventris' first suffixes (-εύς, -τήρ etc.) are correctly identified.

Having fixed the value of -εύς, -ῆρες, -ῆρος, Mr. Ventris was able to identify eighteen nouns of the -εύς type, together with some other words of the same form which he calls personal names. Of the eighteen nouns, thirteen are entirely unknown in later Greek; and some are so baffling that Mr. Ventris himself offers no explanation of their form or meaning. The rest—*χαλκεύς*, *ἱερεύς*, *κναφεύς*, *κεραμεύς* and *βασιλεύς*—are all to some extent ambiguous, owing to the confusion of *r* and *l* and of voiced, unvoiced, and aspirated stops and owing to the omission (whether postulated here or not) of continuant consonants before a stop. Not one of them can be regarded as confirmed by the context in which it occurs. There are eleven supposed examples of agent nouns in -τήρ, of which two are fragmentary endings without roots (i.e. -te-re alone); nine are unattested in Greek and inexplicable, and two are known. The two known words are *ιστήρ* and *στατήρες*, both to some extent ambiguous in form owing to the omission of *r*, *s* and the confusion of *l* and *th*, and neither confirmed by the context. The corresponding series in -τρια or -ταρα contains five words, of which one is actually attested (but only in Eustathius), two are not attested themselves but associated with attested masculines in -τηρ (both infrequent in later Greek), and two are unknown. And so on. More than half of all the nouns listed by Mr. Ventris occur in his article for the first and only time in the history of the Greek language; others are raked from the pages of Hellenistic authors and supported by references to Hesychius. Some, like *ἐντεσδόμος* 'armourer' and *μελλι-δάμπτρες*, presumably 'wives-to-be', are so absurd in both form and meaning that none but an enthusiast could accept them for a moment.

Surely it is clear that Mr. Ventris has gone astray at the very beginning. He took a list of words—not a very long list—with three variant endings; one of these endings occurred sometimes at the beginning of other words, but not very often; another was one of thirteen signs that formed the penultimate sign in all the words in the list. On this evidence alone Mr. Ventris identified his -εύς declension, the vowel *u*, *we*, *wo*, and twelve signs in the *e*-series. It would have been a miracle if he had been right. That he was *not* right, the thirteen unattested words among his -εύς nouns clearly show. It is not hard to think of many other combinations of sounds that might have suited the conditions: e.g. -ξ, -κς, -κας, and the series *Tā* in the penultimate syllable.

If Mr. Ventris could proceed from his eighteen -εύς nouns and give us a list of words beginning with *eū-*, he might alleviate, although he would not remove, our doubts about his original discovery. In fact, he does claim to have found ten personal names with initial *Eū-* and two with initial *Eūpu-*. One must remember, of course, that the sign *e* as such did not occur among the -εύς nouns but was



separately identified. It is not therefore a case of initial *e-u*-confirming final *-e-u*; we are concerned with initial *e-u* and final *Te-u*, and *u* is the only common factor. The *εϋ*-list, however, is at first sight impressive. Unhappily two of the ten *Εϋ*-names violate Mr. Ventris' own rules of orthography, and one of the two *Εϋρϋ*-names depends on the 'Arcadian' trick of *ο* for *α*. Some of the other names are only legible at all through the latitude provided by Mr. Ventris' rules of spelling. Some, on the other hand, admit an embarrassing number of interpretations; for what is the use of writing *e-u-po-ro* for *Εϋπορος*, if you cannot then distinguish this *Euporos* from *Εϋφορος*, *Εϋβορος*, *Εϋβολος*, *Εϋβωλος*, *Εϋφρων* (and *Εϋφρώ*), or from other persons whose names might be written in exactly the same way? There is, however, one vital objection that can be laid against all Mr. Ventris' personal names; that the contexts in which the names occur give no reliable indication that these are personal names at all. In view of this, it may be concluded, from the simple form of each of the names and the marked frequency of the signs that compose them, that their resemblance to personal names need be no more than mere chance. It is certainly not enough to prop up the rickety framework that underlies Mr. Ventris' *-εϋς* declension. And belief in the *Εϋ*-nouns is severely shaken when in addition to those listed by Mr. Ventris in his article there appear strange forms such as *e-u-qo-ne* and *e-u-de-to-ge*.

From the *-εϋς* terminations Mr. Ventris moved on to another group, *-ιος*, *-ια*, then to *-τηρ*, *-τρια*, and so on. He kept on building up little collections of words that share one suffix or another. Each collection contains one or two known words within the category in question—generally rather short words, each consisting of two, three, or four signs. And one or other of these short words is, of course, the starting-point of the identification, the primary conjecture. Other words with the same termination are added to the list. Some few of them turn out to be reconcilable with known words or nearly so; others can be declared personal names. The residue are given a hypothetical form and a hypothetical meaning, and are marked with asterisks, obelisks, and question-marks. The reader of Mr. Ventris' article is then asked to accept the entire collection as Greek.

It is clear that throughout this process the element of conjecture is increasing and not, as one might expect, diminishing. There is no confirmation of the *-εϋς* nouns, or of *-ιος* adjectives, or of *-τηρ*, *-τρια* nouns. Each suffix in turn is a card which forms a new partition or ceiling in an ever-growing house of cards. But however big the house may grow, it is still made of cards, delicately balanced one against the other. The identified suffixes lean on each other; they do not *verify* each other. The very large number of unidentified and unexplained words gives warning that the limit of the structure has almost been reached.

What Mr. Ventris has given us by his transcription is not in fact the Greek language but a language of his own making. It is a strange language, which looks like Greek because he has been careful to provide it with a selection of Greek suffixes. Since he has shorn most words of the whole or part of their inflection and some even of part of their suffixal element, he has been able to dispense almost entirely with the requirements of Greek syntax. And by devising spelling-conventions of primitive simplicity, he has ensured that the syllables preceding the suffix of each word may occasionally be intelligible as Greek word-stems. He has given us first of all a specious interpretation of short, often fragmentary, texts, which, because they are fairly plentiful and repeat the same few words over and over again, may be thought by the inexperienced to confirm each other. They do not in fact serve this purpose. Apart from the words and simple phrases which he took as the basis for his first guesses, he has not given us a single text consisting of several words in direct sequence that makes sense from beginning to end as a Greek phrase or sentence. Instead, we get texts like the so-called Demeter text which I have already quoted or the following from Knossos: . . .

*a-ra-ru-ja a-ni-ja-pi wi-ri-ni-jo o-po-qo ke-ra-ja-pi o-pi-i-ja-pi* CHARIOT (2?)  
*i-qi-jo a-ja-me-no e-re-pa-te a-ra-ro-mo-te-me-no po-ni-[-*

Not even Mr. Ventris' ingenuity will turn this into Greek.

Many scholars may have accepted Mr. Ventris' hypothesis, or may have decided not to oppose it, because they are impressed by his references to statistics. He tells us, for example, that 'a very severe discipline' is imposed on the earliest stages of a decipherment by the use of a grid; he even says that if the initial moves are wrong it should be 'quite impossible' to force any part of the texts into showing the slightest conformity with the vocabulary or grammar of a known language. The suggestion that long lists of figures stand guard behind these pronouncements is enough to scare anyone who, like myself, is naturally afraid of arithmetic. In this case, however, we may easily banish our fears. We have seen the early stages of the decipherment. We have found that the amount of calculation involved is inconsiderable and that its results are wholly unconvincing; the early stages of the decipherment were in this respect allowed to run riot. Moreover, we have seen how Mr. Ventris *does* force texts into conformity with the grammar and vocabulary of Greek, and how the texts successfully resist this process. Accordingly, we shall not be easily scared when Mr. Ventris tells us that the correspondence of a list of words with the names of four Greek deities seems 'ensured by astronomical odds against coincidence'. The fact is, as we have seen, that none of these names can be accepted with confidence and that the list as a whole is far from plausible. What



we have here are three words and part of a fourth which happen to resemble either the names or the surnames of certain Greek deities. All things considered, it seems likely that a coincidence has occurred here. And this seems the more likely when we set the four names of deities against the vast number of texts of which Mr. Ventris can make no sense at all. In terms of statistics and calculable probabilities Mr. Ventris' hypothesis has very little to commend it.<sup>3</sup>

When all the texts have been transcribed according to Mr. Ventris' table of values, there is hardly any Greek to be seen. What little Greek there is has been put there by Mr. Ventris himself, through his identification of *-e-we*, *-i-jo*, *-me-na*, *-te-re* and *pa-te*, *ma-te*, etc. It would be vain to search among the remaining texts for substantially more Greek than has so far been discovered, for the simple reason that Mr. Ventris did not put any more in.

Three years have passed since the publication of Mr. Ventris' article. During that time he and others have often repeated the claims made in that article and have sought to interpret more texts in accordance with the rules set forth in it. The new interpretations are seldom attractive and never convincing; the Appendix to this article contains a detailed refutation of one of the best known among them. It should be stressed that, in spite of all recent work in this field, the 1953 article is still the sole foundation of the decipherment. The passage of months and years may seem to have given authority to Mr. Ventris' theory, and absence of criticism may be thought to have justified all his conclusions. But in fact the theory is no stronger now than it was to begin with. The objections set forth in the preceding pages are, I suggest, of fundamental importance. If Mr. Ventris can show that they are unsound or irrelevant, his theory may be allowed to stand; if not, it must be revised very drastically or rejected altogether.

## APPENDIX

When Mr. Ventris offers to interpret one of the longer texts in Linear B script, it is sometimes easy to refute him. For example, he transliterates a word as *ta-ra-nu* and says that it is equivalent to Gk. θράνυς; and he points to an accompanying ideogram which, he says, represents a footstool. The obvious answer to this is that the ideogram really is a drawing of a flat pan with two handles and that the text therefore belongs to the well-known group of texts concerned with domestic utensils.<sup>4</sup> Again, from a tablet found at Mycenae Mr. Ventris produces a list of plant-names referring to condiments and medicines. But three of the names depend upon our disregarding Mr. Ventris' own rules of orthography,<sup>5</sup> and there are other words which cannot be reconciled with a botanical context. It seems very likely that this list of plant-names is the result of coincidence.

The task of disproving Mr. Ventris' interpretations is often laborious; if we were to examine each of them in detail we should need a great deal of space. Accordingly, I have chosen one text in which the application of Mr. Ventris' values to the syllabic groups seems to give good sense and in which there is a close correspondence between the syllabic groups, on the one hand, and the ideograms and numerals, on the other. This text has probably done more than anything else to convince classical scholars that Mr. Ventris' decipherment is right. If his interpretation of it can be disproved, there is little chance that any of his other interpretations will be able to stand.

The text of the tablet is as follows:

(line 1)	<i>ti-ri-po-de</i>	<i>ai-ke-u</i>	<i>ke-re-si-jo</i>	<i>we-ke</i>		☞
	<i>ti-ri-po</i>	<i>e-me</i>	<i>po-de</i>	<i>o-wo-we</i>		☞
	<i>ti-ri-po</i>	<i>ke-re-si-jo</i>	<i>we-ke</i>	<i>a-pu-ke</i>	<i>ke-re-a<sub>2</sub></i> [	
					<i>ka-u-me-no</i> [	
(line 2)	<i>qe-to</i>					☉
	<i>di-pa</i>	<i>me-zo-e</i>	<i>qe-to-ro-we</i>			☉
	<i>di-pa-e</i>	<i>me-zo-e</i>	<i>ti-ri-o-we-e</i>			☉
	<i>di-pa</i>	<i>me-wi-jo</i>	<i>qe-to-ro-we</i>			☉
	<i>di-pa</i>	<i>me-wi-jo</i>	<i>ti-ri-jo-we</i>			☉
	<i>di-pa</i>	<i>me-wi-jo</i>	<i>a-no-we</i>			☉

<sup>3</sup> Mr. Ventris says: 'coincidence seems insufficient to account for . . . *e-te-wo-ke-re-we-i-jo*, which on values and orthography determined beforehand (and out of 200 billion possible permutations of syllables in an eight-sign word) so exactly yields the patronymic *Ἐπειφωδεφής*'. But given 200 billion variations, anything may happen. Those who take the trouble to decipher the rest of the tablet Sn. 01 will find the context far less impressive than Mr. Ventris indicates.

<sup>4</sup> The ideogram bears only a superficial resemblance to a footstool in Mycenaean art. The words accompanying *ta-ra-nu* are turned by Mr. Ventris into fanciful descriptions of footstools, which do not endure close scrutiny. The series of texts in which *ta-ra-nu* occurs contains several ideograms of vessels, none of furniture.

<sup>5</sup> The three offending words are *ka-ra-ko* for *ga-ra-ko* (βληχών, γληχών), *ka-da-mi-ta* for *ka-ra-mi-ta* (καλαμίνθη), and *ma-ra-tu-wo* for *ma-ra-to* (μαράθος). These amount to a third of the list, and are sufficient to discredit the rest. Also in this text are the expressions *ka-na-ko e-ru-ta-ra* and *ka-na-ko re-u-ka*, sc. κυδίκος ἐρυθρά and κ. λευκά. These have been identified with the *κνήκος*, *Carthamus tinctorius*, and κ. ἄργιος, *C. leucocaulos*, of post-classical Greek. Note, however, (i) that the epithets ἐρυθρά, λευκή are not applied to *κνήκος* by Greek authors; (ii) that *C. tinctorius* has yellow or yellow-brown flowers, and *C. leucocaulos* yellow (not white) flowers in a hood or cup of whitish sepals; (iii) that *C. tinctorius* produces by a complicated process a red dye, *C. leucocaulos* produces no dye at all; and (iv) that neither they nor any other *Carthamus* can be reckoned as condiment or medicament.



Mr. Ventris' interpretation (in its most restrained form) is:

τρίποδε	?	?	?	two tripods, two-handled
τρίπους	?	πόδες?	οἰῶφης	one tripod, two-handled
τρίπους	?	?	?	[one tripod, ?-handled]
?			?	three jars, two-handled
δέπας	μέζοε (sic)	τετρώφες		one pot, four-handled
δέπας	μέζοε	τριῶφες		two pots, three-handled
δέπας	μέφιον	τετρώφες		one pot, four-handled
δέπας	μέφιον	τριῶφες		one pot, three-handled
δέπας	μέφιον	ἀνώφες		one pot, without handle

*I.e.* 1. 1 deals with *tripods*, with double or single handles, and the rest with *vessels*—mainly of the δέπας-type, and these large or small, and having four handles, three handles, or none at all. The words for *tripod* and *vessel* appear in the singular and dual inflections.

In this text, eight or nine words appear to be satisfactorily explained. These are: *ti-ri-po*, *di-pa*, *me-zo-e*, *me-wi-jo*, *o-wo-we*, *qe-to-ro-we*, *ti-ri-jo-we*, *a-no-we* and *po-de* (?). If we include the inflected forms *ti-ri-po-de*, *de-pa-e*, *ti-ri-o-we-e*, the number rises to eleven or twelve. And if we add also forms which are simply repeated, the total is nineteen or twenty words. Against this list, we set eight or nine words that cannot be satisfactorily explained. These are: *ai-ke-u*, *ke-re-si-jo*, *we-ke*, *e-me*, *a-pu-ke*, *ke-re-a<sub>2</sub>*, *ka-u-me-no*, *qe-to*, and *po-de* (?). If repetitions are included, the number will be ten or eleven. Thus on one reckoning *one-half* of the text is explained and *half* unexplained; and on the other reckoning *two-thirds* are explained and *one-third* unexplained.

It is true that Mr. Ventris and others have interpreted *ai-ke-u* as Αἰγεύς, *ke-re-si-jo* as χρήσιος, Κρήσιος, and *we-ke* as ἔχει, etc., and that, in defiance of the word-division, *ke-re-si-jo we-ke* has been made into Κρησιοφεργής. But none of these suggestions passes above the level of conjecture, and for most of them, even as conjectures, there is little or nothing to be said. Mr. Ventris has on occasion identified *e-me* with Gk. ἡμι-, but this carries no weight; and the equation of *po-de* with πόδες seems to create more problems than it solves. There is no proof that *a-pu-ke* and *ka-u-me-no* form one word, or that, if it did, ἀποκεκαυμένος (Arcadian ἀπυ-) would suit the context. *Ke-re-a<sub>2</sub>* can hardly be σκέλεα. Accordingly, we shall not allow these proposals to divert our attention from the fact that at least a third, and perhaps a half, of the entire text *cannot* be read and understood according to Mr. Ventris' decipherment. And this is a large proportion, enough to cause us to examine the half or two-thirds that *can* be understood with great care.

It is well that we should remember the multiplicity of phonetic values attached to each word on Mr. Ventris' hypothesis. It looks at first sight as if this Pylian text could be read straight off without any difficulty; and we might well overlook the fact that the particular interpretation that we are asked to accept is only one possibility out of many. One reason why the transcription looks simple is that ll. 2 and 3 contain few stop consonants, and these few are taken at their obvious value—the unvoiced, unaspirated stop; thus *p* in *di-pa*, *ti-ri-po*, *q* in *qe-to-ro-we*, *t* in *ti-ri-po*, *ti-ri-o-we*, *qe-to-ro-we*. But in fact the values *p*, *t*, *q* are merely symbolic; each stands for a variety of consonants and consonant-groups. And the vowels attached to them are also symbolic, being long or short or (by spelling convention) mute. And even the continuants (*m*, *n*, *r*, *w*, *y*), although they admit fewer variants than the stop consonants, are by no means stable. If we apply these variations to the words in the text we find that *ti-ri-o-we* might be interpreted in 5760 different ways and *qe-to-ro-we* in 92,160 ways. Even so short a word as *di-pa* could mean about 300 different things. If, then, we consider any two or three words together, the number of variants obtained by multiplying together the individual totals for each is truly enormous. And if we took into account *all* the possible values of *all* the words in the text, the final figure might well be described as 'astronomic'.

We are thus reminded that, if any Pylian had written a text according to Mr. Ventris' evaluation of the signs and with his spelling conventions, it would be a miracle if any reader were to recognise what that text meant. I hasten to admit that in the present instance the reader might be able to recognise the nature of the subject-matter from the ideograms and to recognise some of the words, and also, that, as a native speaker of early Greek, he would be able to reject, whether by instinct or by intelligence and training, many possible readings that would be foreign to the language and to the context. Nevertheless, when all allowances of this kind have been made, the number of variants that remained to perplex him would be numbered in thousands, not in tens or digits.

Elsewhere Mr. Ventris has often to search among the less-obvious phonetic variants to find a plausible interpretation, and we are thus made aware of the vast number of alternatives that exist. Here it happens that the most obvious and direct evaluation of the signs is the one that makes sense. For all that, alternatives do exist, and in such numbers that, if this particular interpretation could be proved correct, Mr. Ventris would be more than usually fortunate.

If Mr. Ventris' hypothesis were right and if his interpretation of the Pylian text were also right, we might fairly assume that the word-stems and word-endings in it would, at least occasionally, be confirmed elsewhere. We might find, for example, that the stem *ti-ri-* was identifiable either in synonyms (τρίτος, τριῶν, τρίχα) or in homonyms (τρίζω, τριῶ) or in suffixes (μέτριος, πάτριος).



The suffix *-o-we*, if it occurs elsewhere, might be compounded with other numerals or with descriptive adjectives, or it might be intelligible as a spelling of *-όφεις*, *-οφεν*. Let us see what happens. The following words are transcribed with Mr. Ventris' values from the original form as given in Mr. Bennett's *Index*.

<i>o-wo-</i>	cf. <i>o-wo-to</i> , <i>o-wo-ze</i> , <i>o-wo</i>
<i>qe-to-ro-</i>	cf. <i>qe-to-ro-po-pi</i> , <i>qe-to-ro-no</i> , <i>qe-to-ro-po</i>
<i>ti-ri-</i>	cf. <i>ti-ri-to</i> , <i>ti-ri-se-ro-e</i> , <i>ti-ri-po-di-ko</i> , <i>ti-ri-sa-ta</i> , <i>ti-ri-ti-jo</i> , <i>ti-ri-ti-ja</i> , <i>ti-ri-jo</i> , <i>ti-ri-jo-pa<sub>2</sub></i> , <i>ti-ri-no</i> , <i>ti-ri-ti</i> , <i>ti-ri-da-ro</i>
<i>a-no-</i>	cf. <i>a-no-po</i> , <i>a-no-zo-jo</i> , <i>a-no-qo-ta</i> , <i>a-no-qo-ta-o</i> , <i>a-no-wo-to</i> , <i>a-no-ke-wa</i> , <i>a-no-ke-we</i> , <i>a-no-de-jo-si-wo</i> , <i>a-no-no</i> , <i>a-no-ra-ta</i> , <i>a-no-ze-we</i>

A few words in these lists might be understood as Greek, but none without difficulty. E.g. *ti-ri-to*, which occurs about twenty-five times, looks like *τίριτον*; but it never occurs in a context where *τίριτον* is probable. *Qe-to-ro-po-pi* means *τετραπόφι* to Mr. Ventris; but *τετραπόφι* is a monstrosity. *A-no-wo-to*, where it occurs, can hardly mean 'having no handle', and *o-wo-to* does not seem to mean 'having a handle'. In all cases where a meaning seems possible it is entirely unconfirmed.

There are other indications no less disquieting. In the limited range of Linear B texts available to us, initial *a-no-* is almost as common as initial *άνω-*, *άνωω-*, *άνου-* are in the entire corpus of classical and post-classical Greek literature. It is plainly improbable, however, that in the period following the Dorian invasion so many words with this simple beginning should have vanished from the Greek language. Phonetic and morphological decay of this kind is unprecedented. But we shall find presently that Mr. Ventris' theory requires it not only for *a-no-* but also for *-ro-we*, *-To-we* and for *-po-(de)*, *-pa*, and for other syllables as well.

If we consider not only initial *qe-to-ro-* but all words beginning with the syllable *qe-*, we find that there are twenty-five words in this category. None of them, apart from the three with *qe-to-ro-*, looks like Greek, and it can be stated with some confidence that one or two words amongst them, e.g. *qe-re-me-e*, *qe-ra-di-ri-jo*, could not by any means be twisted into Greek.

Turning to suffixes, we find the following frequencies:

<i>-ro-we</i>	cf. <i>a-ro-we</i> , <i>pa<sub>2</sub>-ni-ro-we</i> , <i>de-ro-we</i> , <i>a-ra-ro-we</i> , <i>ai-ta-ro-we</i> , <i>a-ko-ro-we</i> , <i>]ro-we</i>
<i>-ro-we-e</i>	cf. <i>a-ko-ro-we-e</i>
<i>-To-we</i>	cf. <i>u-po-we</i> , <i>a-do-we</i> , <i>a-qi-zo-we</i> , <i>ko-ra-o-we</i> , <i>?-ko-we</i> , <i>]ko-we</i>
<i>-To-we-e</i>	cf. <i>qi-ko-we-e</i> , <i>?-ko-we-e</i>

The total number of words in this list is sixteen, and it might be augmented if the final syllables of *po-ro-we*, *sa-ro-we*, *a-ko-ro-we*, *to-we*, *a-re-ro-we*, *o-ro-we* were known. Of these sixteen, no less than seven have final *-ro-we*, and one has final *-ro-we-e*. But in Greek there are no words ending with *-ρώνης*, and only one word, *ἀμφώνης*, in a single occurrence, with *-ώνης*. Even if we took into account *-ρόεις*, *-όεις*, and any other suffix of like form that actually existed in later Greek, we should not be able to redress the balance. For most, if not all, of the words listed above are obviously not Greek; and it would be no more than wishful thinking to associate *a-ro-we* with *ἀρόω* or *a-ko-ro-we* with *ἀγρός*.

The *-po*, *-po-de*, *-pa* suffixes yield similar results:

<i>-po</i>	cf. <i>a-ro-po</i> , <i>a<sub>2</sub>-ro-po</i> , <i>ka-na-to-po</i> , <i>]na-po</i> , <i>po-po</i> , <i>o-wi-po-po</i> , <i>da<sub>2</sub>-ru-po</i> , <i>pa<sub>2</sub>-i-po</i> , <i>me-sa-po</i> , <i>e-po</i> , <i>a-no-po</i> , <i>e-wi-ri-po</i> , <i>wa-po</i> , <i>te-o-po</i> , <i>ai-ki-po</i> , <i>ka-po</i> , <i>ka-ka-po</i> , <i>u-po</i>
<i>-po-de</i>	cf. <i>da-da-re-po-de</i> , <i>]re-po-de</i> , <i>po-si-da-i-po-de</i> , <i>ki-ka-ne-po-de</i> , <i>ri-po-de</i> , <i>]ki-ri-po-de</i> , <i>]ri-po-de</i> , <i>]po-de</i>
<i>-pa</i>	cf. <i>to-so-pa</i> , <i>e-re-pa</i> , <i>i-pa</i> , <i>sa-pa</i> , <i>]ti-pa</i> , <i>ri-pa</i> , <i>me-la-pa</i> , <i>o-pa</i> , <i>ka-pa</i>

All three suffixes are well attested. Yet it would be a bold spirit who equated any of the words listed above with any known Greek word. Most of them are utterly un-Hellenic in aspect. It is worth noting, too, that none of the words with *-po-de* are extensions of words ending in *-po*, although the *ti-ri-po*, *ti-ri-po-de* relationship would lead us to expect this phenomenon. Nor is there any sign of a grammatical connection between *-po* and *-pa*, such as we should certainly find between Greek *-πος* and *-πα* in classical times.

Still seeking confirmation of Mr. Ventris' readings *ti-ri-po*, *ti-ri-o-we-e*, etc., we examine words made up of syllables that actually occur in the words of the Pylian text. We might look for Greek words in such cases; but we find none. For example, *ti-ri-no*, *e-wi-ri-po*, *pa-to-ro*, *de-ro-we* are as obscure as any of the strange words that can be discovered by applying Mr. Ventris' values to Mr. Bennett's *Index*.

Finally, if we select from the Pylian text syllabic groups such as *ti-ri-* and *to-ro-*, and examine their use in initial, medial, and final position, we find no more Greek words under this condition than we have found hitherto. When *-ti-ri* is final, it produces *po-ro-ti-ri*, *?-ti-ri*, *ja-ti-ri*. With *to-ro-* in the initial position, we have *to-ro-pa<sub>2</sub>*, *to-ro-qo*, *to-ro-wi*, *to-ro-wi-ka*, *to-ro-wo*, *to-ro-no-wo-ko*, *to-ro-wa-so*,



*to-ro-o*, *to-ro-ki-no*, *to-ro-ge-jo-me-no*; and with final *to-ro* we find *da-to-ro*, *pa-to-ro*, *e-ru-to-ro*, *go-e-to-ro*, *ru-wo-to-ro*, *ke-to-ro*, *a-ke-to-ro*, *pu-to-ro*, *re-u-ko-to-ro*, *]we-to-ro*, *ka-to-ro*. Again it is impossible to equate any word in these lists (except perhaps *e-ru-to-ro*) with a known Greek word. The words are numerous enough, but they do not suggest the Greek suffixes *τορ*, *τρο*, *θο*, *θλο* or the root *-τοπος*.

Having reviewed all the evidence set forth above, we are bound to conclude that Mr. Ventris' decipherment of the Pylian text is in no way supported by the application of the same syllables and syllabic groups to other texts. The evidence is far from scanty; in certain cases it is abundant. But the syllabic groups, either in the same position in the word as in the Pylian text or in other positions, stubbornly refuse to yield Greek words. And some of the groups, notably *a-no-* and *-ro-we*, are far more frequent in the Linear B script than their equivalents ever were in classical Greek. Accordingly, those who subscribe to Mr. Ventris' interpretation of this text must acknowledge also the flood of un-Greek words that it carries with it. Either they will have to postulate the occurrence of an un-Greek language alongside Greek in the Linear B documents or they will have to hope that elements which now appear un-Greek may by some miracle be proved to be Greek in the end.

I now turn to examine that part of the Pylian text for which a plausible interpretation has been proposed. And at this stage it is important to remember that a substantial number of words in the text are still unexplained, that the phonetic evaluation of the text as a whole is only one of many variants, and that the phonetic values attached to the syllables and syllable-groups concerned are by no means confirmed in other texts. These are formidable obstacles; but there are worse to come.

First I consider individual words in the text.

1. The word *ti-ri-po(-de)* is applied twice, and was probably applied three times in all, to basins mounted on three-legged stands. This corresponds to the Homeric and classical use of *τρίπους*, and is therefore at first sight acceptable. There are, however, two possible objections. First, Gk. *τρίπους* is as much an adjective as a noun, and some centuries before Homer it might be expected to appear as an adjective rather than as a noun. Secondly, since ll. 2-3 deal with vessels and not with stands, one would suppose the subject of l. 1 to be not the three-legged stands but the basins which they support. We might look to see *λέβης* etc. *τρίπους*, not *τρίπους* alone.

2. Gk. *δέπας* means 'cup', 'goblet'. Such cups were made of metal and generally had one or two handles; very large, ornate cups might have as many as four handles. But the ideograms that accompany the word *di-pa* on the Pylian tablet do not represent cups, but jars or pots. And four of the pots have not one or two 'handles', but three or four; and the fifth has none. Since they are listed with tripods and are few in number, we may perhaps suppose that like the tripods they were made of metal. We may also guess that they were comparable, though not necessarily equal or nearly equal, to tripods in size; i.e. that they were fairly large pots. The fact that they have 'handles' fitted to the rim suits this hypothesis. For a cup or goblet would have large handles, fitted to the rim and side or to the rim and bottom, and a large earthenware jar would have small handles, on the rim and side or on the side only; and small earthenware cups are hardly to be considered here. Only a big metal pot is likely to be held up by means of lugs protruding from the rim.

So the identification *di-pa* = *δέπας* does violence to the facts of the context, so far as they can be discerned. This conclusion is supported by a Knossian tablet which associates the word transcribed *di-pa* with a round, heavy-looking vessel, not at all like a cup. Another Knossian tablet shows a vessel on a tripod stand, with the syllable *di* inscribed upon it. Possibly *di* here stands for *di-pa*; but the vessel once more is not a cup, but an amphora.

In any case, the notion that the first syllable of Gk. *δέπας* might in Mycenaean times be written *di-* is unwarranted. In classical Arcadian *i* occurs for *e* before *n*, and occasionally before *d*. But it is not likely that this tendency began as early as the Mycenaean age or that it ever spread beyond the mountain-valleys of Arcadia. And there is no trace of a shift from *e* to *i* after a dental stop or before a labial stop in any part of the Peloponnese. (Gk. *ἵππος* cannot be admitted as evidence here. It is by no means clear how *ἵππος* developed out of *\*ekwos*, if at all. But in any case the conditions are quite different. On the one hand, there is no by-form *\*ἑππος*, *\*ἔππος* in Greek, and on the other hand, there is no *\*δίπας*.)

3. The word *qe-to* in l. 2 should on the analogy of *ti-ri-po(-de)* and *di-pa* mean 'amphora'. But there seems to be no Greek word that suits this hypothesis. Certainly we must reject on phonological grounds any connection with *πίθοι*. It may be worth noting that the Knossian tablet (already mentioned) which contains the word *di-pa* also contains a word *qe-tu*. According to Mr. Ventris, I suppose, *qe-tu* might be an 'Arcadian' form of *qe-to*. But even if this were so (and it is highly improbable), the Knossian text would not confirm that *qe-to* meant a kind of vessel, any more than it shows *di-pa* to mean 'pot'. It would merely illustrate a second time that *qe-to* and *di-pa* could be used in association with vessels, and it would show that the type of vessel concerned need not always be the same. There is nothing to show that either word did not signify an occupation, a commodity, a measure, or the like.

4. To readers of Mr. Ventris' article, the words *me-zo-e*, *me-wi-jo* are old friends. Mr. Ventris identified them, and also *me-zo* and *me-wi-jo-e*, at an early stage in the process of deciphering the



script. The Knossian texts in which they were first recognised are supposed to give lists and numbers of boys and girls, who are thus divided into 'large' and 'small' categories. It is quite unlikely, however, that children would be so classified, rather than by an age-limit or by an exact measure of height. An exact division by age or height might, it is true, be designated simply as 'large' and 'small'; but this again is unlikely. It is also unlikely that in the Pylian text pots would be marked simply 'large' or 'small'. Homer distinguishes them by a measure of capacity, and that is the obvious way. But in any case, whether in relation to children or vessels, Mr. Ventris' assumption that *me-zo*, *me-wi-jo* mean 'large' and 'small' respectively remains a guess, which the mere multiplication of instances does nothing to confirm.

If *me-zo(-e)* is outwardly unobjectionable as the equivalent of μέζον, μέζω, the same is not true of *me-wi-jo* in relation to μέιον. For the IE base of μέιον is \*mey-, \*mi-. Neither in Greek nor elsewhere is there any sign of a variant \*mew- or \*mewi-. The mere fact that Mr. Ventris' transcription now yields *me-wi-jo(-e)* is no proof to the contrary.

Besides, Mr. Ventris' interpretation of the inflected forms *me-zo-e*, *me-wi-jo-e* will not bear serious examination. In the Pylian text he has to assume that the first *me-zo-e* was intended by the writer to be *me-zo*. I.e., the writer meant to write the neuter singular, but for one reason or another he wrote the neuter dual, which he was to use in the following phrase. This excuse cannot be accepted. Speakers of inflected languages make such slips but rarely; and in the present case correction would have been easy. In fact, the association of *me-zo-e* with both *di-pa* and *di-pa-e* in the Pylian text appears to be in harmony with the use of *me-zo-e* and *me-wi-jo-e* in a number of Knossian texts. The forms with and without *-e* are evidently used alongside each other and without any distinction of number. The natural inference from such cases is not that writers of Linear B constantly confused the singular, dual, and plural inflections but that Mr. Ventris' evaluation of the sign which he renders *e* is mistaken.

In addition to these considerations it should be observed that uncontracted *oe*, *ae*, *ee* are by no means probable in nominal inflections, even in the second millennium B.C.

5. The Pylian text contains four different words ending in *-owe(e)*, occurring six times in all. There are sixteen examples of this suffix elsewhere in the Linear B corpus. In classical and post-classical Greek the suffix *-ῶς* occurs in one word only and on one occasion. I have already remarked on this discrepancy between Mr. Ventris' Greek and the Greek that we know (see above, p. 11).

This *-owe(e)* suffix in Mr. Ventris' transcription is said to represent Gk. *-ῶφες*, *-ῶφες*, a compounded form of οὔς, οὐατος. But in classical Greek compounds formed from this word end in *-οὔατος*, contracted *-ῶτος*, and they occur as early as Homer. Against these compounds we have to set the isolated occurrence of ἀμφῶς, 'two-handled', in Theocritus. Does ἀμφῶς confirm the existence of a series of compounds with final *-owe* a thousand years earlier? It is very hard to believe that it does. For ἀμφῶς seems likely to be a coinage of late epic poetry or even of Hellenistic poetry. It is made by removing the common suffix *-τος* from ἀμφῶτος and replacing it by the more archaic *-ης*. Indeed, the contracted form ἀμφῶτος from ἀμφούατος is almost a pre-requisite for the creation of ἀμφῶς.

We should in any case doubt whether *-owe* is the proper form for a derivative from οὔς in Mr. Ventris' transcription. IE \**-ōus-es* should give early Greek \**-ōuhes*, \**-ou'es*, with the syllabic break after the diphthong *ou*. It is not a case of intervocalic *w* between *ō* and the ending *-es*, but of a true diphthong *+es*. According to Mr. Ventris' rules, strictly applied, the inherited form should be written *-o-u-e* or *-o-u-we*. And there is no reason why the rules should not be strictly applied in this case; for the IE diphthongs are not likely to have undergone serious modification in the Mycenaean age.

6. We have to ask whether the numerical elements prefixed to the suffix *-owe(e)* are correct. Provisionally we may accept *o-wo-* = οἰῶν and *ti-ri-o*, *ti-ri-jo-* = τριῶν; they conform to the rules of orthography. With *qe-to-ro-we* and *a-no-we* the case is not so simple.

In Greek we find ἄουτος 'deaf' (Hesychius), ἄωτος 'without handles' (Philetas, Plutarch, etc.); Homer has ἄουτος 'unwounded'. We also find in Homer ἀνούατος, 'unwounded'. But nowhere do we find ἀνόντος, 'without ears, handles'. It is therefore curious that some centuries before Homer *a-no-we* should occur, apparently in ordinary use, with *n* bridging the hiatus between prefix and stem.

In a word formed from IE \**getwr-* and *-ōuses*, we should expect to find \**τετραῶφες* or, if we were to allow Mr. Ventris' substitution of 'Arcadian' *o* for *a*, \**τετροῶφες*. We do not expect the contracted \**τετρώφες*, *qe-to-ro-we*; for even in historical times *τετρα-* is often uncontracted when followed by a vowel. This is not a matter upon which one would in other circumstances lay much weight. Its importance is increased, however, by Mr. Ventris' insistence on final *-ae*, *-oe*, *-ee*, all uncontracted.

Lastly, it should be remarked that *o-wo-we*, οἰῶφης, 'with single handles', differs in sense from the other words ending with *-owe(-e)*. It refers to the *form* of the handles, not to the number of handles on each vessel. Although it is quite possible in theory that οἰῶς and οὔς could be compounded in this way and that the compound should have this meaning, it is not at all likely that such



a compound would be used alongside others meaning 'three-handled' and 'four-handled'. There would be no way of telling that the word did not mean 'one-handled'; and, given the existence of a series 'three-handled', etc., we should expect a different kind of expression or a different kind of compound to denote the form of the handles (e.g. *σὺν ἀπλοῖς οὐασιν*, *ἀπλ-οὔατος* or *σὺν οὐασιν ἀπλοειδέσιν*). On this account the presence of *o-wo-we* in the text does not support the validity of the other three adjectives, but rather tells against it. It reminds us that *-we* is a very common suffix in Linear B documents and that *-To-we*, too, is well-attested. The appearance of *To-we* four or five times in one of Mr. Ventris' transcriptions is indeed not very remarkable; it might arise from many causes and need have nothing to do with lugs or handles.

If individual words in Mr. Ventris' transcription are open to criticism, the transcription as a whole is in no better case. Each entry on the tablet is supposed to give the name of a vessel, and most entries in addition state the number of handles on the vessel and indicate its size. But the ideograms which accompany the written entries also specify the kinds of vessel and the number of handles on each. The only matter on which the words give information and the ideograms do not is the size of the vessels. On the other hand, the words do not say how many vessels of each kind there were, but the ideograms are accompanied by figures which serve this purpose. We can only wonder that the writer did not have at his disposal an ideographic sign for 'large' or 'small' and that he did not think it necessary to write out the words corresponding to his figures. It is more than surprising, however, that in so short a document there should be so much duplication of phonetic script and symbols as there is. In these circumstances ability to read and write seems largely wasted.

This Pylian tablet is presumably, like so many others, a record of goods held in, due to, or paid from the palace. Yet it fails to say who was the owner, creditor, or debtor; it does not say where the goods came from, where they were, or where they were going; it does not suggest what purpose they served or on what occasion they were listed; it omits all mention of materials, contents, values. It would be useless to seek any information of this kind in the obscure portions of l. 1 (*Αἰγεύς, Κρήσιος*, etc.); for it is a safe assumption that each entry in l. 1, like those in ll. 2-3, is complete and self-contained and contains nothing that could illuminate the other entries. We cannot avoid recognising that, on Mr. Ventris' interpretation, the tablet is a mere catalogue of vessels without any apparent context.

The most curious feature of all is the writer's insistence on handles. One might imagine that handles were a criterion of size; but this is evidently not the case here, since the vessels in ll. 2-3 are already divided into 'large' and 'small', and the classification by handles cuts across this division. We should in any case suspect the validity of a list that has no one-handled or two-handled pots but knows only those with three or four handles or none at all. Vessels lifted by one man's hands usually have one or two handles; those intended for two men to lift have either two handles or four. Three handles are a rarity.

It is important to observe the deficiencies and inconsistencies of Mr. Ventris' text as well as its actual content. A single-handled tripod is apparently worth mentioning (*o-wo-we*), but a double-handled tripod is not; and whether the third tripod had handles or not, evidently no attempt was made to say so in words. Again, the amphora handles, apparently single, are not called *o-wo-we*; and indeed they are not mentioned. Thus out of nine items, three are not described in terms of handles; but at least two of the three had handles, and they seem to have differed from each other chiefly in the shape of these handles (see below). Accordingly, it appears that handles are far less conspicuous in this text than we might at first suppose, and they are least conspicuous where there is seemingly most reason to mention them.

Our examination of Mr. Ventris' interpretation has given the following results. Of the three words that are supposed to refer to a kind of vessel, *qe-to* and *di-pa* are unacceptable, and *ti-ri-po(-de)* is not above suspicion. Both the adjectives *me-zo-e* and *me-wi-jo* are to be rejected without reservation. The 'handle'-epithets present a more impressive appearance than the rest; but the *To-we* suffix is very far from convincing, and the forms *a-no-we* and *qe-to-ro-we* are suspect. And there is a serious discrepancy in meaning between *o-wo-we* and the rest. Thus every single word in the text is open to criticism, and in most cases the criticism is so severe that there is little chance left of Mr. Ventris' proposal being right. In these circumstances the individual words do not support each other; the case is rather that the faults discovered in each unite to throw discredit on all. When we take into account also the apparently purposeless character of the text as a whole, its repetitions, omissions, and inconsistencies, we cannot but conclude that Mr. Ventris' interpretation is seriously mistaken. To reinforce this conclusion we have at hand the three points that came to our notice at the outset—the unexplained portion of the text, the variable value of the transcription within the limits of Mr. Ventris' own rules, and the absence of any confirmation in other texts for the phonetic values applied here.

All that remains to support Mr. Ventris is a vague but prevalent feeling that, even if his interpretation is virtually meaningless, somehow it 'hangs together' and that this degree of intelligibility could not depend on mere coincidence. For my own part I should be content to confront those who are of this opinion with the linguistic arguments which I have already advanced. But in order to demonstrate that such obsession is unreasonable, I shall now show, first, that an alternative inter-



pretation of the context of this tablet at least as probable as that of Mr. Ventris can be achieved and, secondly, that the working of coincidence in Mr Ventris' interpretation is not so extensive or so complicated as it might seem at first sight.

It is evident that Mr. Ventris has failed to produce a convincing synthesis of words and ideograms, or even to account adequately for the ideograms alone. The question arises whether there is a better explanation of these matters.

The tripod-handles and amphora-handles differ noticeably from the pot-handles. They are circular grips, and project outwards from the side or shoulder of the vessel. Evidently they are true handles, to be grasped with the hand. On the first tripod the handles are double; i.e. each ring has a dent in its circumference and so makes a grip for two hands together. The handles of the second tripod are plain circles and give a hold for one hand only. We may infer that the first tripod is of a large, heavy type that might need two men to lift it. The second tripod would then be smaller and lighter, lifted by one man with both hands or by two men, one on each side, using one hand each. The form of the third tripod is unknown. The amphora ideogram in l. 2 has rings like those of the second tripod. We may guess that it represents a vessel about as heavy as the second tripod, or at any rate not heavier. If so, the tablet as far as the beginning of l. 2 will represent vessels in descending order of magnitude.

The handles on the pots are set along the rim, not on the side or shoulder; and they project upwards, not outwards. They seem, moreover, to be oval or eye-shaped rather than round. And when they occur, it is in threes and fours. Now these pots might be very heavy, and more than one man might be needed to move them; or, if they could be moved by one man, it might be convenient for him to have more than two grips to catch hold of from time to time. But then it would be hard to understand why of three vessels which, according to Mr. Ventris, were about equal in size one should have four handles, another only three, and a third none at all.

If, however, the vessels from beginning to end of the tablet, and not only from the beginning to the amphora ideogram, are arranged in descending order of magnitude, we may suppose that all the pots are smaller than the tripods and amphorae, or at least no bigger. We may further conjecture that the pots with four lugs were the largest, those with three of middle size, and those with no lugs the smallest. In this case we shall have to leave unexplained the repetition of the four- and three-lugged types in ll. 2-3; the cause of this repetition might lie in a difference of contents, value, or the like.

Now it is possible to explain the lugs not by reference to Mr. Ventris' suffix *-owe(e)* but from their actual shape and arrangement. These lugs are not, I suggest, handles but 'eyes' or rings for the attachment of chains or cords, Gk. κῑκος rather than οὖς. The pots were to be kept hanging from a beam or hook or they were to be raised and lowered on the end of a cable. The number of lugs is limited to three or four by the need to support the vessel securely and maintain its balance; two might not serve, and five or more would be too many. The size of the pots can hardly be determined accurately from the number of lugs, but those with four may well have been both larger and more open at the top than those with three.

Pots without lugs would simply be grasped with the hands and made to stand upon a flat surface or grill.

The Pylian tablet on Mr. Ventris' interpretation is concerned with vessels of three kinds and with their handles. But we now say more accurately that it deals with tripods of two sizes, having different handles, with amphorae, and with pots of three sizes, distinguished by a varying number of rings and chains. How are we to relate the words of the text to these objects? We shall not, of course, attempt an alternative transcription and alternative interpretation to those of Mr. Ventris; we shall limit our attention to the external evidence of the writing.

The first thing to be noticed is the relationship between the words transcribed *qe-to* and *qe-to-ro-we*. Since both words are used in connection with vessels of similar, though not identical, shape, it seems reasonable to suppose that *qe-to-ro-we* is a case-form, derivative, or compound of *qe-to*. We must not lose sight of the possibility that *qe-to* and *qe-to-* are homonyms without any common meaning, but we may provisionally believe that they are related.

We may then identify, also provisionally, the *-ro-we* of *qe-to-ro-we* with the 'suffix' of that form which is recorded seven or eight times in Linear B documents (see above, p. 11). And, remembering that *-we* is a common final syllable, we shall cease to attach any special significance to its appearance in this text after *o-*, *jo-*, *o-*, *no-*; the occurrence elsewhere of *-do-we*, *-po-we*, *-zo-we*, *-ko-we* sets our minds at rest on this score.

Next we may compare the words which Mr. Ventris renders *ti-ri-po(-de)* and *ti-ri-o-we-e*, *ti-ri-jo-we*. We shall not assume that the two words last-mentioned are variants of the same word, the *j*-glide being inserted or removed at random, or that the syllables transcribed *ti-ri-* are a prefix meaning 'three' or that they refer to legs and rings. We need go no farther than to guess that all three words are case-forms, derivatives, or compounds of a stem *ti-ri-*.

So by a few simple observations and inferences we have formed the outline of an interpretation of the text. We find the two elements *ti-ri-* and *qe-to-* (cf. also *ke-re-* in l. 1) running through the whole document, apparently in some sort of gradation or subordination. Without venturing to say



that Mr. Ventris' transcription of the words is correct, far less to reinterpret his transcriptions, we may claim that this outline offers the possibility of a really significant interpretation in which the ideograms and figures would be *equivalent* to the words but not a *repetition* of them. And in this respect it is clearly superior to Mr. Ventris' interpretation.

The extent and importance of coincidences in the original text of the Pylian tablet and in Mr. Ventris' transcription of it should be calculated accurately, and neither over-estimated nor under-estimated. It is in any case important to keep the repetitions and correspondences in the original separate from those which depend on the method of transcription. The following occur in the Linear B form of the text (for convenience I represent the words in transcription):

Word.	No. of occurrences.	Remarks.
<i>ti-ri-po(-de)</i>	3	twice (at least) with a tripod
<i>di-pa</i>	5	always with a pot
<i>me-zo-e</i>	2	twice with <i>di-pa</i> and pot
<i>me-wi-jo</i>	3	three times with <i>di-pa</i> and pot
<i>qe-to-ro-we</i>	2	twice with <i>di-pa</i> and four-lugged pot
<i>ti-ri-</i>	2	twice with <i>di-pa</i> and three-lugged pot
<i>o-wo-</i>	1	with ring-handles on a tripod
<i>a-no-</i>	1	with lug-less pot
<i>-we(-e)</i>	6	five times with <i>di-pa</i> and pot, once with <i>ti-ri-po</i> and tripod.

Mr. Ventris' transcription produces the following instances in addition:

Word.	No. of occurrences.	Remarks.
<i>ti-ri-o-we(-e)</i>	2	twice with <i>di-pa</i> and three-lugged pot, if the <i>jo</i> of <i>ti-ri-jo-we</i> is equivalent to <i>o</i>
<i>-To-we</i>	6	five times with <i>di-pa</i> and pot, once with <i>ti-ri-po</i> and tripod.

In considering the foregoing table we must bear in mind that mere repetition in a catalogue may signify nothing. If *ti-ri-po* occurs once with a tripod or *di-pa* with a pot, then either of these coincidences repeated a hundred times will hardly reinforce what is already evident, viz, that *ti-ri-po* and tripod are in some way relevant to each other; and so with *di-pa* and pot. They will not show that *ti-ri-po* means 'tripod' or that *di-pa* means 'pot'. Even if *di-pa* occurs alongside a pot elsewhere (as in fact is the case), we shall be as far as ever from proving the identity of the word *di-pa* and the object 'pot'. So also with groups of words. If *di-pa me-zo-e* occurs once, it may well occur twice, and we shall infer nothing from the repetition. *Di-pa me-wi-jo* we shall treat in exactly the same way. From other Linear B texts it is known that *me-zo-e*, *me-wi-jo* often occur together and that they apparently differentiate objects that are otherwise alike; but we can infer no more than that from their conjunction here. Again, if there is attached to each instance of *di-pa me-zo-e* or *di-pa me-wi-jo* a word ending in *-we*, we must recognise that this phenomenon, having occurred once, may in a list be repeated again and again. We shall not jump to any further conclusion concerning these words and phrases. Indeed, since *-we* occurs also in relation to a different kind of vessel with a different kind of handle, we shall be cautious with regard to any such conclusion.

We shall not relax our caution when Mr. Ventris presents us with the two transcriptions *ti-ri-o-we(-e)* and *ti-ri-jo-we* and tells us that *o* and *jo* are mere phonetic variants. There is no reason to expect such a variation within two lines of a short text, even if it were usual from one text to another or from one part of the Mycenaean empire to another. Again, given the frequency of *To-we* in other texts, it is unnecessary to infer that its repeated occurrences here have any *special* bearing on the sense of the words in which it is used or of the text as a whole.

We may fairly exclude from the reckoning the correspondences *a-n(o)-* 'without' (sc. handles) and *o-wo-* 'single' (sc. -handled). The former does not suit the phonological pattern of the Greek language; the latter does not suit the context that Mr. Ventris postulates (see above, pp. 11 and 12f.).

The list of repetitions and correspondences is thus a good deal less impressive than it seemed to be. Indeed, there now remain only two cases for which it might still seem proper to postulate some other cause than coincidence. These are:

<i>ti-ri-</i>	'three'
<i>qe-to-ro-</i>	'four'

Even if all else be disregarded, these two stems together make a formidable appearance. The question is, are they strong enough to overturn all my arguments and vindicate all those put forward by Mr. Ventris?

In considering *ti-ri-* and *qe-to-ro-*, I recall that *-po(-de)* and *-(T)o-we*, with which they are associated, are unconfirmed and improbable, that the apparent contraction of *qe-to-ro-* with *-o-we* is unacceptable on the conditions with regard to contraction and non-contraction laid down by Mr.



Ventris, that the *-ro* (for *-ra*) of *qe-to-ro* is doubtful. I recall also—and this is of more fundamental importance—that initial *qe-* lacks confirmation, and that both *ti-ri* and *to-ro* in initial, medial, and final position are unconfirmed. In these circumstances it would be amazing if *ti-ri* 'three' and *qe-to-ro* 'four' were to prove correct.

In the Linear B corpus ideograms of metal and clay vessels appear very frequently and it can only be an accident that among the many types of vessel represented tripods are rare outside the Pylian tablet under discussion. It must be accidental, too, that there is apparently only one other tablet that shows a pot with lugs on the rim—a pot with three lugs on a fragmentary tablet from Knossos. Three-legged stands and pots with three lugs or four were presumably common enough in cities of the Mycenaean age.

Now the elements *ti-ri*, *to-ro*, and initial *qe-* are well attested. There are many examples of initial *ti-ri*, and even a few of initial *qe-to-ro*. We have already observed, however, that in other texts *ti-ri* and *qe-to-ro* are not associated with 'three' or 'four'.

If, then, we find in this Pylian text *ti-ri* with three lugs and three legs and *qe-to-ro* with four lugs, we have no right to infer that there is an essential connection between these syllabic groups and these numbers. The syllabic groups and the ideograms are alike commonplace. It is remarkable that they should happen to occur together, but the coincidence is not strong enough to support an interpretation of the whole text. Since we have seen that this interpretation is otherwise insecure and verging on collapse, we must conclude that the apparent correspondence between words and ideograms is due to chance. It is, when all is said and done, a simple enough coincidence, of the kind that crops up constantly in the case of simple numbers and simple words.<sup>6</sup>

We have seen that, even without any knowledge of the language of the Linear B script, it is possible to construct an interpretation of this Pylian text that will stand comparison with Mr. Ventris' interpretation. We have seen, too, that the extent of verbal and numerical correspondences in this text is relatively unimportant and attributable to chance. Therefore we may return with confidence to the linguistic arguments that have already been advanced against the Ventris interpretation. If these arguments are well-founded, most or all of that interpretation must fall; and with it must fall much of Mr. Ventris' entire decipherment. If, on the other hand, Mr. Ventris is to maintain both his interpretation of the Pylian text and his system of decipherment, he must find a convincing answer to the linguistic objections to which both are exposed.<sup>7</sup>

I am indebted to Professors A. C. Aitken and A. H. Campbell and to Mr. A. H. Coxon for advice and criticism given to me during the preparation of this paper. Responsibility for the opinions expressed in it is naturally mine alone.

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<sup>6</sup> Although Mr. Ventris has concentrated attention on the *ti-ri*, *qe-to-ro* elements, and on the *-Toue* ending, it is well to consider also the following table of words. There is an apparent relationship between these words in Linear B, but it does not correspond to anything, real or apparent, in Greek.

<i>ti-ri-po(-de)</i>	<i>ti-ri-jo-we</i>	<i>ti-ri-no</i>	<i>ti-ri-to</i>	
<i>qe-to-ro-po-pi</i>	<i>qe-to-ro-we</i>	<i>qe-to-ro-no</i>		
<i>a-no-po</i>	<i>a-no-we</i>	<i>a-no-no</i>		<i>a-no-ze-we</i>
	<i>a-wo-we</i>		<i>a-wo-to</i>	<i>a-wo-ze</i>

<sup>7</sup> The tragic death of Mr. Ventris in a motoring accident was announced as the *Journal* was going to press. The author and editors wish to express their sorrow at this grievous loss to scholarship, and their hope that the new enthusiasm for Minoan studies his work has aroused will continue to bear fruit.



## SOME ATTIC FRAGMENTS: POT, PLAQUE, AND DITHYRAMB

THE only common factor shared by the fragments of vase and votive plaque discussed below is that all are Attic. The plaque fragments seemed to deserve individual treatment outside the general study of votive plaques from the Athenian Acropolis on which I am engaged. The vase fragment with the Marsyas scene which is published here with some comment on the possible literary treatment of the myth is in the possession of Mr. John Leatham. To him I am indebted for permission to study and publish the piece, and to Mme. S. Karouzou, Prof. O. Broneer, the Keeper of Antiquities in the Ashmolean Museum, and the Director of the Thorvaldsen Museum for permission to publish pieces in Athens, Oxford, and Copenhagen.

### I. MARSYAS AND MELANIPPIDES

The fragment which is here illustrated on Plate I, 1, 3 is at present in the keeping of Mr. John Leatham. Unfortunately its provenience can be defined no more nearly than as 'from Greece'. On the photograph reproduced on Plate I, 3 I have added in white the painted details on the sherd which the camera could not record. It is from the rim and wall of a calyx crater,<sup>1</sup> part of whose lip decoration, a palmette scroll, is preserved. From the figure scene below we see the upper part of an Athena who holds her spear upright in the right hand while her arm hangs at her side, perhaps with the hand on the rim of a shield which rested on the ground beside her. She wears an aegis over her peplos, and an Attic helmet with raised cheek-pieces. She is looking to her left and slightly down towards a figure before a tree with white painted leaves. The figure is apparently seated, and his snub nose shows that he is a satyr or silen, and not a young one to judge by his shaggy white hair. The two are identified by the white painted inscriptions in the field, ΑΘΗΝΑΑ and ΜΑΡΣΥΑΞ, Athena and Marsyas.

The painter is the Kleophon Painter, and many features of the painting on our fragment recall his style and mannerisms. The Athena figure is exactly like the Athena he painted on a vase now in Palermo,<sup>2</sup> and details of the helmet<sup>3</sup> and the drawing of the silen's head<sup>4</sup> are equally familiar. Were more preserved, I think the vase would be judged one of his better pieces, and one of his earliest, perhaps before 425 B.C.

The scene is one which, with a number of variants, had first become popular in Attic vase painting by the beginning of the last quarter of the fifth century.<sup>5</sup> The story is well known: Athena had invented or discovered the flute, but disgusted at the distortion of her face while she played it she threw it away and the silen Marsyas picked it up. This is the moment that Myron chose for his famous Athena and Marsyas group on the Athenian Acropolis,<sup>6</sup> but it is not the one most favoured by the vase painters,<sup>7</sup> who seem to have preferred its sequel. For Marsyas, soon adept with his new instrument, challenged Apollo to a musical contest which the silen lost, and as a result was flayed for his presumption. Athena's earlier interest in the fortunes of the flute make her often a spectator of the contest, as she is here, although in other representations of this scene she regularly stands between the rivals<sup>8</sup> and not to one side. Her expression shows her feelings for Marsyas (seemingly not shared by her chuckling gorgoneion). Apollo must have stood to the right, and there were probably other spectators also. As Marsyas' head is raised he is probably not playing the flute, but since vase painters<sup>9</sup> knew a version of the story not met in the literary tradition, in which the silen also plays Apollo's lyre or cithara,<sup>10</sup> this may explain the figure here.<sup>11</sup> The scene can hardly be that of Apollo playing and Marsyas inactive, also known on vases, as Athena's glance clearly indicates who is the main figure. The silen's hair is usually painted black, but here the Kleophon Painter has given him a shaggy white poll.

The variety of the representations of the Marsyas story in Attic art in the fifth century is more remarkable than is their number. There is Myron's group of little before the middle of the century illustrating the encounter with Athena, and vase representations of this scene as well as tableaux

<sup>1</sup> The dimensions of the fragment are, length 11.5 cm., height 10.0 cm. The lip was slightly offset and there is a reserved line in, half-way down the fragment. The diameter of the vase at the base of the lip was about 28.5 cm.

<sup>2</sup> ARV 786, no. 29, Mingazzini *Apoteosi*, pl. 4, 1.

<sup>3</sup> E.g. on his vases illustrated in CR VIII 212-22.

<sup>4</sup> Especially AJA XLV 600, fig. 5 (ARV 785, no. 16).

<sup>5</sup> Roscher s.v. 'Marsyas', Beazley, *EVP* 75 f., Metzger, *Les Représentations* 158 ff., Wegner, *Das Musikleben der Griechen* 18 f., 166. For literary references RE s.v. 'Marsyas'.

<sup>6</sup> Lippold, *Griechische Plastik* 139.

<sup>7</sup> Metzger, *op. cit.* 163 n. 5.

<sup>8</sup> *Ibid.* 161 f., nos. 11, 12, 15, 16.

<sup>9</sup> Beazley, *op. cit.*, Metzger, *op. cit.* 161 f., nos. 15-17.

<sup>10</sup> The difference between lyre and cithara is, of course, important, but not significant in the argument I develop here. Except in descriptions of particular instances my use of the word 'lyre' should be taken to embrace both these stringed instruments.

<sup>11</sup> As the Kleophon Painter's satyr cited above (note 4) who has thrown his head back while playing.



of all subsequent episodes in the story. It would appear then that the source of this interest is more likely to be literary than a work of art; a play or poem, parts of which are illustrated, rather than a piece of, say, major painting, which would then be simply copied. The work which has been mentioned in this context<sup>12</sup> is the lost 'Marsyas' by the dithyramb writer Melanippides of Melos.<sup>13</sup> It is in fact the only work dealing specifically with this subject of which we have evidence in this period, and could well have been composed before the middle of the fifth century. Melanippides was also known for his musical innovations in the traditional structure of the dithyramb, which seem to have involved the introduction of lyric passages or solos (*ἀναβολαί*) into work normally accompanied only by the flute.<sup>14</sup> It would be difficult to think of a plot better suited to illustrate or introduce this innovation than that of Marsyas, a contest between flute and lyre in which the latter prevails. The episode with Athena might appropriately open the action before an Athenian audience, and be followed by the contest with Apollo, which Athena would attend as she does on some vases, and with perhaps the Muses as judges:<sup>15</sup> in all a fine dramatic agon. We could well imagine that, defeated by the lyre, Marsyas himself handles the instrument, as we see him on some vases and perhaps on our fragment; not, however, in a spirit of bravado but submissive acknowledgement of defeat, and this, I would suggest, might well have formed an integral part of Melanippides' story and perhaps even an alternative ending to the flaying. The flaying story was known, as a local legend, to Herodotus (VII 26), but does not appear to have been illustrated in Greek, or at least Attic, art until the fourth century,<sup>16</sup> while the lyre-playing Marsyas is met already more than once in the fifth century. The flaying, too, may have owed much of its subsequent popularity and acceptance as part of the canonical story to its representation in monumental art, both painting and sculpture, and indeed the same may be said of the Athena-Marsyas story immortalised by Myron. An explanation which has been offered for the popularity of this Apollo and Marsyas episode in the mid-fifth century is that it reflects the enmity of Athens and Boeotia at this time, and represents symbolically the victory of the Athenian lyre over the Boeotian flute.<sup>17</sup> But the introduction of the Phrygian silen Marsyas into this situation is particularly unhappy, while on the other hand, as champion of the traditional dithyramb,<sup>18</sup> which may even be Phrygian in origin,<sup>19</sup> his participation in a contest which indirectly involves fundamental details of its composition is quite easy to understand. If any national rivalry is to be reflected in the tale the fresh memory of the Persian Wars would no doubt heighten an Athenian audience's appreciation of the discomfiture of an oriental by Apollo: and even so, as Melanippides' introduction showed, this Dionysian 'spirit of dithyramb' Marsyas owed his instrument to a concession by the city's goddess, Athena, who abandoned it as ill-suited to her dignity.<sup>20</sup> It cannot have been long after 'Marsyas' was produced that the Athenian theatre-going public was reminded by Sophocles in the 'Ichneutai' that Apollo, too, owed his lyre to another god, Hermes. Silenus and the satyrs were among the first to hear its note, but hardly with enthusiastic approval.

Melanippides chose Marsyas to illustrate the contest of old and new which his innovations involved, and his solution of it was a compromise,—the converted Marsyas plays the lyre; we may even suppose that this episode in the story was his own invention, to suit his purpose, and it is, of course, possible that he preferred the Conversion to the Flaying as an end to the action. He, too, may have been responsible first for the conflation of the Boeotian and presumably Eastern accounts of the discovery of the flute, by Athena and Marsyas respectively.<sup>21</sup> Marsyas' conversion and the success of the new-style dithyramb may be illustrated also by a vase painting of the third quarter of the fifth century which figures a lyre-playing silen whose name is Dithyramb (*ΔΙΘΥΡΑΜΦΟΣ*: Plate III, 1)<sup>22</sup>; while another vase which has recently been recovered from the mud of Lake Comacchio shows a silen dressed as Apollo playing the cithara for the delight of his master Dionysos and with Hermes in the audience.<sup>23</sup>

<sup>12</sup> Metzger, *op. cit.* 1631, gives references.

<sup>13</sup> Schmid-Stählin, *Geschichte der griechischen Literatur* 492 f.

<sup>14</sup> *Ibid.* 493, Pickard-Cambridge, *Dithyramb, Tragedy and Comedy* 53-8 and *Dramatic Festivals* 79 (where the argument about the introduction of lyric passages found in his former book is not mentioned). Buschor sees evidence of earlier rivalry in music and instruments in dithyramb or satyr play (*Satyrspiele und frühes Drama* 85).

<sup>15</sup> As they are on the Mantinea relief (Lippold, *op. cit.* 238, *RE* s.v. 'Marsyas' 1990) and as, on vases, some of the spectators are identified. Other gods may also have been judges, as they attend also on the vases (Metzger, *op. cit.* 158-62 *passim*). The three women in the audience on the Paestan vase in the Louvre by Assteas, who was fond of taking his inspiration from the stage, may be Muses (Trendall, *Paestan Pottery* 42 f., fig. 23, pl. 11 c, *BSR* XX 6, no. 63).

<sup>16</sup> First illustrated, so far as we know, by Zeuxis (Pliny, *NH* XXXV 66, on a pinax taken to Rome); also on an Attic vase of around 330 (*JHS* LIX, pl. 4-6), and fourth-century Italian vases (Beazley, *Greek Vases in Poland* 76, Trendall, *Paestan Pottery* 43). The famous group of the bound Marsyas

and the Scythian is Pergamene of the third century (Lippold *op. cit.* 321 f., Bieber, *Sculpture of the Hellenistic Age* 110 f.).

<sup>17</sup> Huchzermeyer, *Aulos und Kithara* 60 f., Roos, *Die tragische Orchestik* 220 ff., Metzger, *op. cit.* 167, Wegner, *op. cit.* 155 f.

<sup>18</sup> Rizzo thought that the dithyramb might have suggested to the vase painters the subject of a lyre-playing satyr (*MA* XIV 61 f.).

<sup>19</sup> Pickard-Cambridge, *Dithyramb, Tragedy and Comedy* 14-18.

<sup>20</sup> Page, *Greek Literary Papyri* I 26 ff.

<sup>21</sup> Cf. Preller-Robert, *Griechische Mythologie* I 223.

<sup>22</sup> ARV 698 Group of Polygnotos no. 56, Pickard-Cambridge, *op. cit.* 11 fig. 3.

<sup>23</sup> Arias, *Illustrated London News* December 4, 1954, 1015, fig. 8 (Beazley, *Paralipomena* to ARV 2389: by the Boreas Painter). The scene on a crater in Erlangen, which Metzger interprets as of a 'Dionysiac' Apollo (*op. cit.* 188 no. 41a, Grünhagen, *Antike Originalarbeiten*, pl. 14) may also be associated with the Marsyas story. It shows Apollo seated, with a lyre at his feet, attended by Eros and women, and with a white-haired silen seated below him and holding a flute in either hand. On cithara-playing satyrs see also Roos, *op. cit.* 227 ff.



Melanippides' musical innovations provoked both Pherecrates and Democritos of Chios<sup>24</sup> to the attack, and his dithyramb seems also to have popularised a story which Attic artists were not slow to illustrate in almost all its stages. As well as the silen's encounter with Athena we see represented on fifth-century vase paintings his performance with his flute, once with the lyre at his feet ready to be taken up,<sup>25</sup> and elsewhere his essay with lyre or cithara as well as Apollo's performance with his own instruments. An early fourth-century crater in Berlin may be, as it were, a commentary on the conversion, for upon it we see Marsyas interrupting a lyre concert of Apollo and the Muses with a gesture which might be interpreted as a request to handle the god's lyre himself.<sup>26</sup> The earliest of all these representations was Myron's group of Athena and Marsyas on the Athenian Acropolis,<sup>27</sup> and it is perhaps no mere chance that the only passage from Melanippides' dithyramb which is preserved<sup>28</sup> describes exactly the moment the sculptor chose:

ἀ μὲν Ἀθάνᾳ  
τῶργαν ἔρριψέ(ν) θ' ἱερᾶς ἀπὸ χειρὸς  
εἶπε τ'· "ἔρρετ' αἰσχεα, σῶματι λύμα,  
ἐμὲ δ' ἔγω (γ' οὐ) κακότεστι δίδωμι."

Might it not be that the group itself is the proud dedication of Melanippides on the occasion of his successful and novel 'Marsyas'?<sup>29</sup>

## II. ACROPOLIS PLAQUES BY RED-FIGURE ARTISTS

The finest painted votive plaques from the Athenian Acropolis are of the decades before and after 500 B.C., and many of them the work of leading red-figure artists; but they are not all of them in the red-figure technique, as something of the same conservatism seems to have dictated the decoration of votive plaques as it did many traditional or ritual vase shapes, such as loutrophoroi and Panathenaic amphorae. It is some of the plaques of this period and their painters that I wish to discuss here.<sup>30</sup>

The fragmentary plaque illustrated in Plate II 1 comprises two pieces in Athens (Akr. 2590: Graef-Langlotz I, pl. 109, shows the right-hand fragment) and one in Oxford (1927.4602: cf. Beazley, *JHS* LI 53).<sup>31</sup> As can be seen, the Oxford fragment links the two in Athens. The plaque was coated with a creamy-buff ground which carries the decoration, and it was pierced at the top by two holes neatly cut before the plaque was fired to admit thongs. On it appears the upper part of the figure of Athena with her spear raised, in a pose well known on other dedicatory plaques in Athens and on Panathenaic amphorae, and which may represent a Pisistratid statue of Athena on the Acropolis. Behind her we read the dedication Πολίτας ἀνέθεκε (Fig. 1a).<sup>32</sup> No incision is employed (except in the eye), and we may suspect a red-figure artist at work, but the piece must be classed with the other black-figure plaques. On the unslipped back of the plaque the artist had roughly sketched out, first with a blunt tool on the soft clay, then with his brush, the figure of the Athena he was to execute fully on the other side, perhaps to get the scale right for the size of the plaque (Fig. 1b, Plate I, 2).

The drawing of the plume of Athena's helmet in almost all its details and disposition of colour very closely resembles that of the running warrior on another, larger Acropolis plaque which may have formed part of a balustrade or some semi-permanent structure.<sup>33</sup> It is figured in Graef-

<sup>24</sup> Pickard-Cambridge, *op. cit.* 54-6. Plato's rejection of Marsyas' instrument (*Rep.* III 399e, quoted by Metzger, *op. cit.* 167) is in keeping with his apparent disapproval of mixed Dionysiac composition (*Laos* III 700d, quoted by Pickard-Cambridge, *op. cit.* 58 f.). Unpopularity of the flute in Athens around the middle of the fifth century is suggested by Wegner, *op. cit.* 103.

<sup>25</sup> Metzger, *op. cit.* 161, no. 14. The vase in Serajevo (*ibid.* 159 n. 1, Bulanda, 39 fig. 54, Beazley, *EVP* 76) shows Marsyas seated before a tree, his head on his hands, holding his flutes listlessly; at the left stands Athena, and over the silen's head a Nike flies towards the goddess (Apollo, no doubt, stood beyond her).

<sup>26</sup> Metzger, *op. cit.* 159, no. 4, pl. 22, 2: Furtwängler thought he might also have been holding his flute (*Beschreibung* no. 2638).

<sup>27</sup> Marsyas alone with his flute appears on a vase of about the same date (cf. Beazley, *EVP* 76).

<sup>28</sup> In Athenaeus XIV 616c. *Anthologia Lyrica* (Diehl) II<sup>2</sup> 196, no. 2.

<sup>29</sup> We do not know whether the 'Marsyas' won a prize, though it is not unlikely. The usual dedication would be by the choregos, and would be a tripod, its position dependent on the festival involved. Some dithyrambs were performed in the Lesser Panathenaea (Pickard-Cambridge, *op. cit.* 10), which might involve a dedication on the Acropolis (for earlier citharode dedications there see Raubitschek, *Dedications from the*

*Athenian Acropolis*, nos. 84, 86). Others have thought Myron's group an ex-voto for a successful dithyramb, cf. Picard, *La Sculpture* II 232. The presence of a tripod in one of the vase scenes of the contest may imply a stage success (Ruvo 1093, *MonIned* VIII pl. 42; on the neck of the vase the silen plays the flute, on the body he plays the cithara and a prize tripod stands between him and Apollo); cf. Metzger *op. cit.* 29 f.

<sup>30</sup> Some general remarks on votive plaques can be found in *BSA* XLIX 183 ff.

<sup>31</sup> The complete dimensions are, height 15.5 cm., width 18.0 cm. (original width around 23.0 cm.), thickness 1-1.2 cm. Red for the outline of the plume and helmet peak, brown for the spear and snakes, white for the arm, and chequer pattern on the crest. The white ground overlaps the edges. In Plate II 1 the lower part of the left-hand fragment is cut away; it bears no part of the figure drawing, and the inscription which continues on it is not well enough caught by the camera. The join is not quite exact in the photograph owing to a slight difference in the scales of the reproductions.

<sup>32</sup> In Graef-Langlotz the final *epsilon* was not read.

<sup>33</sup> Akr. 1037. I have mentioned this piece with further references in *BSA* XLIX 201, no. 8. The inscriptions on it are of no concern to us here, and their scale is, of course, much greater than those on the Athena plaque; both were painted in red. For a comparable treatment of the plume in red figure compare the Troilus Painter's amphora in the Vatican (*ARV* 190, no. 1, Gerhard *AV*, pl. 126: the plume is outlined in red).



Langlotz II, pl. 80, but the drawing in *AE* 1887, pl. 6 should be consulted for the colour.<sup>34</sup> The major part of the plaque appears here in Plate II, 2. The pupil of the eye is rendered in the same way on both plaques, as a circle of paint with an incised ring in it, but there is unfortunately no opportunity for closer comparisons between the Athena and the running warrior.<sup>35</sup> There are then at least some grounds for believing that they may be the work of the same painter, and Beazley has in fact associated the two in *Paralipomena to ARV* 78 f.

The warrior plaque was associated with the red-figure painter Euthymides by Stuart-Jones in 1891,<sup>36</sup> and is styled by Beazley 'related to' that artist.<sup>37</sup> So we have the Athena plaque *possibly* by the same painter as the warrior plaque, which is *possibly* by Euthymides. The uncertainty disappears, I think, when we remember the name of the man who dedicated the Athena plaque, Pollias, none other than Euthymides' father whom the painter mentions in signatures on four of his vases, and whom he persists in spelling in the old-fashioned manner with only one *lambda*.<sup>38</sup> We know how

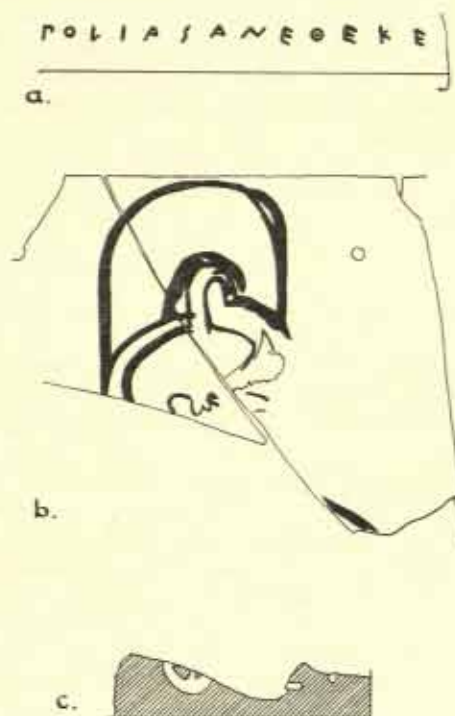


FIG. 1.—(a) PAINTED DEDICATION ON AKR. 2590.  
(b) PAINTED SKETCH ON THE BACK OF AKR. 2590 AND OXFORD 1927. 4602.  
(c) INSCRIPTION FROM AKR. 1042.

Scale 2:3.

Pollias spelled his own name because he was a sculptor whose signature appears or is plausibly restored on no less than six Acropolis dedications.<sup>39</sup> In the light of this connection the details of the two plaques are also found to tally well enough with the known red-figure work of Euthymides.<sup>40</sup> As this was a family of artists, what could be more natural than that the son should paint and inscribe a plaque for his father to dedicate?<sup>41</sup>

As no black-figure vases have been convincingly attributed to Euthymides we are denied the

<sup>34</sup> Reproduced by Hoppin in *Euthymides and his Fellows* 90, fig. 16.

<sup>35</sup> The pointed elbows and profiles of the nape of the helmet are similar; they are not, of course, helmets of the same type.

<sup>36</sup> *JHS* XII 380.

<sup>37</sup> *ARV* 934. Rumpf *MuZ* 65 cites it as by the Bowdoin Eye Painter. Hoppin (*op. cit.* 91) quotes as a parallel the shield device on the Munich amphora (*ibid.*, pl. 2); the other vase he mentions is not assigned by Beazley (*ARV* 914). As well as on this plaque *Megakles kalos* appears on four vases, one of them by Euthymides (*ARV* 26, no. 10), who, so far as we know, did not honour any of the other youths he mentions on vases with the epithet.

<sup>38</sup> *ARV* 24 ff. nos. 1, 4, 9, 15 (on no. 9 he also omits the first *omicron*).

<sup>39</sup> Raubitschek, *Dedications from the Athenian Acropolis* 522 f. discusses and gives references; on only one inscription is the double *lambda* extant, so one might argue that this could be a mistake and that the six examples painted by his son, with one

*lambda*, are correct. He may be the sculptor of bronzes, Pollios, mentioned by Pliny (*NH* XXXIV 91). Robert suggested that artists only mention their fathers' names if they were artists also (*RE* s.v. 'Euthymides', cf. Raubitschek, *ÖJh* XXXI Beiblatt 38).

<sup>40</sup> Compare the Munich amphora in its fine photographs in Lullies and Harmer *Griechische Vasen*, pll. 17-23, noting ears, earrings, Korone's hair, the forearm of Pirithous. Euthymides also regularly painted his vase inscriptions in red; the *sigma* on the Athena plaque is rather less angular than we might expect.

<sup>41</sup> Just as, if Raubitschek's optimistic restoration is accepted, Euthymides himself may have been taught to sculpt by his father and dedicated one of his own bronzes on the Acropolis (*op. cit.* 168 f., no. 150). Pollios executed a dedication for Kriton, son of Skythes, who may be the known vase painter (*ibid.* 250 f., no. 220). The Etearchos of *ibid.*, no. 221, which may have carried a statue by Pollios, could be the youth named on a vase by Phintias (*ARV* 23, no. 8), the companion of Euthymides.



opportunity of comparing his style on plaques with that on smaller works. The figure on the warrior plaque was over 40 cm. high, and the Athena probably 30 cm.<sup>42</sup> They were therefore both of them undertakings for which the painter was obliged to adapt his usual style, for he had to depict a large figure in a single panel which would catch the eye and hold the attention, and at the same time he had the opportunity to indulge in greater detail on his larger 'canvas'. Any painter who turned from the decoration of vases to plaques was also dealing with a more easily manageable object which could lie on a table or stand on an easel, not the curved surface of a vase which he perhaps had to balance on his knees. Finally, he was painting an object which would be on display, something designed to grace a public sanctuary or temple, not a private shelf, wall, or dining-table, and he might therefore be expected either to give of his best in a manner unfamiliar on his vases, or, unequal to the challenge and the scale, produce the stiff, fussy drawing we see on many plaques. It is probably for these reasons that many scholars once doubted whether the painters of clay plaques were the same artists as those who decorated vases. This at least has been disproved; but there are still many fine votive plaques which have not been assigned to painters because considerations of detail have not always supported the verdict of first impression. Not, of course, that all plaques were large, or all would-be masterpieces, but I think we may be forgiven for paying more attention to those which as decoration and as works of art do not suffer in comparison with the bronze and marble dedications.

Of the great red-figure artists of the Early Archaic period we have seen that Euthymides decorated plaques in the old, black-figure technique in keeping with the tradition of such dedications on the Athenian Acropolis. A contemporary of his, perhaps a little younger, also painted black-figure plaques and red-figure vases. He has been called the Cerberus Painter, but one of his plaques tells us his name, Paseas.<sup>43</sup> Euphronios, too, may have painted black-figured plaques, and Peters<sup>44</sup> has suggested that Akr. 2514 (Graef-Langlotz I, pl. 103) may be from his hand. There are also other fine black-figure plaques of this period from the Acropolis, not as yet attributed to known vase painters. The painters were not slow to use the new red-figure technique for plaques, but the few extant examples from Athens and Eleusis show that it never enjoyed the same popularity for this type of dedication as the sixth-century black figure. It may be that already the competition of the painters of wooden panels was being felt.

There are among the few early red-figure plaques from the Acropolis two particularly fine specimens, one of the period of Euthymides and Paseas, the other somewhat later, and it is to these that we turn now. Of both unhappily the greater part is missing.

The first is Akr. 1042, four of whose five fragments are illustrated in Graef-Langlotz II, pl. 82, and to which Miss Pease added another fragment from the excavations on the North Slope of the Acropolis (S-5-1, *Hesp* IV 238 f., no. 27, fig. 12). It was painted about 500 B.C., and the scene it carries is of the Judgement of Paris;<sup>45</sup> not one which could be considered flattering to the goddess to whom the plaque was dedicated, but the painter hardly thought of that when he or his client chose it. At the left the apprehensive Paris (1042 a, b) moves away carrying his lyre but looking back towards Hermes (1042 a, b: HE[P]MΩ),<sup>46</sup> who hurries after him and restrains him with his outstretched right arm: he seems almost over-dressed with chlamys and chiton, caduceus and sword,<sup>47</sup> and his winged petasos. The preserved part of the petasos, with the flying strands of his hair over his shoulder, suggests that he, too, is turning his head towards the goddesses who follow him, but of this we cannot be certain. The two heads turned back in this way would help to lend balance to a composition which might otherwise have been uneasy with the two figures moving away from the centre. The leading goddess, of whom nothing is preserved, must have been Hera, in a position befitting her seniority, and behind her came Athena shouldering her spear (1042 c). At the rear comes Aphrodite (1042 c, d<sup>48</sup> and North Slope S-5-1: ΑΦΡΟΔΙΤ[Ε] closely attended by Eros (1042 e and North Slope S-5-1: ΕΡΩΣ), who may be holding a wreath above her head.<sup>49</sup> This, incidentally, must be one of the earliest representations of Eros as a youth, and a rare one of him fully clothed.<sup>50</sup>

The whole plaque must have measured at least 80 × 30 cm. Its upper edge, which projects over the main scene, like eaves,<sup>51</sup> is painted red on its underside and bore an inscription in cream-white paint, probably the dedication.<sup>52</sup> In the one corner preserved appears a large hole which was

<sup>42</sup> That is to say, twice as large, and half as large again as the figures on his amphorae. The figures on his painted stand in the Agora (ARV 26 no. 15, *Hesp* V 59 ff. figs. 1, 2) were about as tall as the Athena.

<sup>43</sup> Boardman, *JHS* LXXV 154 f.

<sup>44</sup> *Studien* 56, cf. Rumpf, *Mus* 74.

<sup>45</sup> Clairmont, *Das Pariser Urteil* 47 K. 129.

<sup>46</sup> Beazley, *Greek Vases in Poland* 12 n. 4, *AJA* LII 338.

<sup>47</sup> Clairmont, *op. cit.* 106 lists five Judgements at which he wears a sword, and cf. *ibid.*, 21 n. 37.

<sup>48</sup> This carries the drapery covering her upper leg and bottom, and part of the characteristically decorated hem to her mantle.

<sup>49</sup> The earliest appearance of Eros at the Judgement, except perhaps for the Etruscan vase, Clairmont, *op. cit.* 19 f. K. 9 bis

(A2 1883 307 f.), where one of the two Erotes present holds a wreath over Aphrodite's head; cf. Clairmont, *op. cit.* 112.

<sup>50</sup> Alternatively, to restore HIMEP[O] makes the representation no less remarkable; but Himeros never operates on these occasions without his brother.

<sup>51</sup> Cf. *BSA* XLIX 191 f.; Benndorf, *Griechische und Sicilische Vasenbilder*, pl. 5, 3 has the profile.

<sup>52</sup> The fragment 1042 a, which bears the letters ON I have not seen. A drawing of what appears on 1042 e is reproduced here in Fig. 1c. It could be restored . . . ΘΕΝ, the final letter being rather cramped. This is not likely to be any part of ἀντιθέμι, or an Ἀθηναία foreshortened. It could be an ablative ending as Ἀλομικέθεν (Raubitschek, *op. cit.* Index 532 f., Graef-Langlotz, II no. 1324).



certainly cut to admit a nail, and from the position of the dedicatory inscription on the overhanging upper edge it must have been designed to be set at least as high as eye level.

Langlotz saw in the profile of Paris' head the influence of the painter Oltos, and in the drapery that of Euthymides. Miss Pease, adding the North Slope fragment, remarked that details pointed rather to Phintias<sup>53</sup> and Robertson has associated it<sup>54</sup> with a vase of which Beazley writes 'related to Phintias, and might be a late work of his.'<sup>55</sup> I think the plaque was certainly by Phintias, but that in looking for detailed comparisons we must remember that we are dealing again with figures appreciably larger than those on the painter's vases and with something which was more of a bespoken showpiece than his usual orders. The close-set lashes of Paris' eye we find on two of Phintias' larger and more elaborate vases<sup>56</sup> and the wreath with the floral centre is of a type he particularly<sup>57</sup> favoured. The simple ear, profile, hair,<sup>58</sup> nostril, and leg<sup>59</sup> and arm markings are readily paralleled on his work, and the lyre is a slight elaboration of his usual pattern for the instrument.<sup>60</sup> The border of Aphrodite's drapery is unique, but it is in Phintias' work among all of this period that rows of dots are to be found in a comparable position.<sup>61</sup> To the scale of the work we may attribute the fussy multiple lines outlining chiton and sleeves,<sup>62</sup> and the very fine shading of the chiton,<sup>63</sup> so close as to give the impression of a uniform brown wash.

The second red-figure plaque is Akr. 1047. When complete it was probably as high as Phintias' Judgement of Paris, but as the number of figures involved is not clear we cannot divine its original width. Graef-Langlotz illustrate (II, pl. 82) four of the five fragments there described. There is one other fragment among the Acropolis pottery in the National Museum at Athens which belongs, and one added by Miss Pease from the North Slope excavations (A-P 194, *Hesp* IV 239, no. 28, fig. 12). The same source yielded two other fragments (A-P 3267 a, b) since Miss Pease's study, one of which joins 1047 a, and these appear here in Plate III, 2. Of the distinguishable figures we have, from the left edge of the plaque, the drapery from the back of a figure facing right (A-P 194), and before it part of a woman facing left (1047 a and A-P 3267 a) with her braceleted forearm slightly raised; her right arm must have been raised still higher, and this unusual position and the lie of the drapery folds above and below her left arm can be explained only by the fact that she is holding in her arms a child, whose swaddling shawl we see. Back to back with her stood a citharode (1047 a). Another figure, also facing right (1047 d), holds a slender curved object and a ribbon-like strand. Langlotz saw here an Artemis stringing her bow, though this does not seem the gesture, the bow tip is not hooked, and her bowstring is badly tangled.<sup>64</sup> However, as the only other figure we can distinguish is divine, a garlanded Hermes facing left holding a caduceus and stretching out his right arm, the identification of both an Apollo and Artemis for the other two figures may be correct. Four other small fragments carry parts of drapery (1047 b, e, A-P 3267 b and one other). It remains only to identify the goddess carrying a child. Leto she clearly cannot be, but there are earlier representations of Aphrodite carrying children on an Acropolis vase and plaque: on the former the goddess alone is identified by an inscription,<sup>65</sup> but on the plaque<sup>66</sup> the children are named, Himeros and Eros. There, as on our red-figure plaque, they are still wingless babies, though we know from the Phintias plaque discussed above that Eros was already represented as a winged youth. We have then an assembly of gods. The edge of the plaque is preserved on one side only (A-P 194), and there a projecting ridge has been broken away outside the pomegranate chain which frames the picture (*cf.* 1047 c). We can confidently restore similar ridges on the other three sides, much in the fashion of the later naiskos plaques.<sup>67</sup>

Our piece was painted by an artist strongly influenced by the work of his greater contemporaries, particularly the Berlin Painter, and though I do not know other works from the same hand, this was clearly an artist of no mean ability, though not quite a master. His dependence and generation are revealed in his treatment of hair, features, and drapery,<sup>68</sup> but his execution of detail

<sup>53</sup> *Hesp* IV 239.

<sup>54</sup> *JHS* LXX 32 n. 45, where it is seen as a link with the early Myson vases (*ARV* 171 f.), which it closely resembles, though it is apparently earlier than them.

<sup>55</sup> *ARV* 23 bottom.

<sup>56</sup> For the eyes *cf.* *ARV* 22, nos. 2 and 4 (Hoppin, *op. cit.*, pl. 26, *FR* III 234).

<sup>57</sup> *CF.* *ARV* 22 nos. 1, 4 and 5 (Hoppin, *op. cit.*, pll. 31, 28, *FR* III 234).

<sup>58</sup> *CF.* Apollo on *ARV* 22 no. 2 (Hoppin, *op. cit.*, pl. 26).

<sup>59</sup> *CF.* *ARV* 22 nos. 1 and 7 (Hoppin, *op. cit.*, pll. 31, 27).

<sup>60</sup> *CF.* *ARV* 22 nos. 5 and 7 (Hoppin, *op. cit.*, 105 pl. 28).

<sup>61</sup> *CF.* *ARV* 22 nos. 5, 6 and 7 (Hoppin, *op. cit.*, 118, pll. 28, 27).

<sup>62</sup> *CF.* *ARV* 22 nos. 1 and 2 (Hoppin, *op. cit.*, pll. 31, 26): a feature of the more scrupulous work of several contemporary painters.

<sup>63</sup> *CF.* *ARV* 22 no. 2 (Hoppin, *op. cit.*, pl. 26).

<sup>64</sup> Prof. C. M. Robertson has suggested that the 'bow' may be the end of a helmet crest, the figure holding up the helmet rather as does a youth on a vase by the Chicago Painter (Beazley, *Greek Vases in Poland*, pl. 22).

<sup>65</sup> Akr. I 603 pl. 29 (an assembly of gods).

<sup>66</sup> Akr. I 2526, pl. 104 and *cf.* 2494, pl. 101 (an assembly of gods). On Akr. 2526 see also Papaspyridi A Δ XI 106; Seltman, *BSA* XXVI 89 f., 104; Beazley, *JHS* XLIX 262 f. (*ibid.*, pl. 15, 24 for another Aphrodite carrying a child) and *JHS* LII 180. *CF.* Robinson, *AJA* LX 7.

<sup>67</sup> *CF.* *BSA* XLIX 192.

<sup>68</sup> E.g. the outlined lock before Aphrodite's ear. The narrow opening to the chiton sleeve is not uncommon in the Kleophrades Painter's work (Beazley, *Der Kleophrades-Maler*, pll. 27, 28.2, 30.7), and the hem pattern is unusual but similar to that on some of the same painter's vases (Hoppin, *op. cit.*, pl. 12, Pfuhl *MuZ* III, fig. 330). The flying strands of hair in a reserved ground and not incised, the short curly hair over the forehead, the long beard whiskers, and the long drawn line of the jawbone are to be found in the work of the Berlin Painter, who seems to be more consciously copied by our artist (for the hair *cf.* Beazley, *Der Berliner-Maler*, pll. 9-12, forehead and beard especially, *ibid.*, pll. 27.3 and 32, chin, *ibid.*, pl. 3 and *AJA* XXXIX, pl. 9: further on the Berlin Painter, Robertson, *JHS* LXX 23 ff.).



falls short of the best contemporary work—Aphrodite's hand is weakly drawn with no definition of the knuckles, the hair is represented in an unusual manner, perhaps a misunderstood variation on the hair style we find on the Berlin Painter's work, the fine lines on the chiton are not intelligently applied, the drapery hangs in carelessly rendered folds which are neither precise nor realistic, and throughout the drawing the line is weak and halting. The explanation may lie with the scale of the figures, which would not have dismayed either the Kleophrades or Berlin Painters, but which might have troubled an artist accustomed to working only on smaller vases or cups, and such he may be. The date should be around 490.

### III. A TOTENMAHL PLAQUE

The fragment of plaque, Athens NM 1244 (Plate III, 3) deserves consideration apart from all other known Attic red-figure plaques, both for its unusual representation and for its place of finding. The scene which appears on it is one familiar enough on stone reliefs, the 'death feast', probably better known in its German title *Totenmahl*; and in its decoration and shape this plaque more than any other illustrates how, towards the end of their history, clay plaques imitated rather stone originals than the painted wooden plaques which were the companions, though not in every way the inspiration, of their archaic predecessors.<sup>69</sup> The naiskos plaques,<sup>70</sup> with their raised side ridges and pediments crowned with acroteria, are further examples of this dependence of votive plaques of the fifth and fourth centuries on stone models, a particularly fine example being the Ninnion plaque at Eleusis, which is the best preserved of them.<sup>71</sup> But unlike the naiskos plaques, NM 1244 owes its decoration rather than its shape to the stone originals, although it, too, has a slight overhang painted with an egg-and-dart pattern at the upper edge, reminiscent of the projecting eaves which are carved above many stone votive reliefs. It was first published in a drawing by Wolters in *AJA* 1896, 145. Only the top left-hand corner of the plaque is preserved, and the whole must have measured over 30 cm. wide by nearly 20 cm. high. Of the painted scene we have, in a square field in the corner, the head and neck of a horse painted white. Below is the procession of a family of worshippers headed by the parents and completed by the four children nicely graduated in size. All wear wreaths, and the mother carries in her right hand a small branch and in her left some small, undefined object, while the father may, from the position of his arm, also have held tokens of worship or offerings. The inscription above, Πανσανίας ἀν[έθηκεν tells us his name, and the fact that it is painted shows that the plaque was bespoke, and so the family may have been represented accurately, at least so far as their number is concerned. On the analogy of stone reliefs we can restore in the right-hand half of the plaque a seated or reclining male figure with a table by him. A good photograph of the fragment has been published by Herbig in *RM* XLII Beil. 14, and it has been dated by Schefold on stylistic grounds to the decade 330–320 B.C.<sup>72</sup>

The similar scenes on stone votive reliefs which show worshippers approaching or attending what is apparently a feast have often been discussed and associated with hero cults, and their particular suitability to Asklepios and kindred healing deities pointed out by Hausmann.<sup>73</sup> The horse's head is generally now explained as a symbol of aristocracy rather than death,<sup>74</sup> as may have been also the sixth-century black-figure horse-head amphorae.<sup>75</sup> We may recall also the earliest figure drawing on Attic pottery, which is of horses<sup>76</sup> as prize possessions, and the social implication of the Greek term ἵππεύς, 'chevalier'.

The sanctuaries in which red-figure plaques were dedicated in Attica are by no means confined to those on the Athenian Acropolis or at Eleusis, as finds in the Agora and on the Pnyx show.<sup>77</sup> They are appropriate, as is any other form of dedication, for any deity or shrine. The plaque under discussion here was found in 1891 during the construction of a house in Aristides Street, a little to the west, that is inside, of an identified stretch of the city wall.<sup>78</sup> Some 350 m. to the south-west<sup>79</sup> were found two inscriptions,<sup>80</sup> of the late third and late second centuries, from a sanctuary of Heros Iatros. Versakes' theory<sup>81</sup> that the foundations of a large building found at the corner of Praxiteles Street, some 200 m. from the inscriptions, are of a temple or some other building in this sanctuary is, as Judeich pointed out, sheer conjecture, but worth noting here, as the remains lie on a line between the find places of the inscriptions and the plaque. Admittedly the distances involved are great, but small objects can travel a long way, and, leaving aside the question of Versakes' foundations, the connection between the plaque and the inscriptions is rendered more plausible by the

<sup>69</sup> Cf. *BSA* XLIX 187 ff.

<sup>70</sup> *Ibid.* 192 n. 95; add a Corinthian example *Corinth* XII, pl. 65, 888.

<sup>71</sup> *AE* 1901 pl. 1.

<sup>72</sup> *UKV* 119.

<sup>73</sup> *Kunst und Heilum* 111 ff. with earlier references.

<sup>74</sup> *Ibid.* 159 n. 425, Nilsson, *Geschichte der griechischen Religion* I<sup>2</sup> 382 f.

<sup>75</sup> Cf. Beazley, *Development* 40.

<sup>76</sup> The earliest, *Kerameikos* IV, pl. 8, 27. Desborough is sceptical about their possible social significance (*Protohistoric Pottery* 301).

<sup>77</sup> Agora P 9468 (*ARV* 14), P 380 (*ARV* 683); Pnyx PN P-87; cf. Berlin 2759 (Beaudouf, *Griechische und Sicilische Vasenbilder*, pl. 4, 2, *ÖJh* I 89, fig. 38, perhaps from the Hephaisteion) and 2760 (from Nola!).

<sup>78</sup> Judeich, *Topographie von Athen* Plan I G. 2 (centre). Wolters, *op. cit.* describes the house as the second on the right coming down the street from the north.

<sup>79</sup> Judeich, *op. cit.* F. 3 (centre).

<sup>80</sup> Judeich, *op. cit.* 379 f. n. 7, Kutsch, *Attische Heilgotter* 48–52, *IG* II<sup>2</sup> 839, 840; for the former see Kirchner, *Imagines* no. 94, pl. 38.

<sup>81</sup> *AE* 1910 267 ff.



obvious suitability of a *Totemahl* votive in the sanctuary of a Heros Iatros. The worship of the latter is attested in Athens,<sup>82</sup> both by the inscriptions mentioned above and from literary sources; the title is met also at Eleusis<sup>83</sup> and can be regarded as the anonymous counterpart to the many named hero-doctors whose cult is practically indistinguishable from that of Asklepois, and who often even claimed him as their father. If this association of the plaque and the inscriptions is acceptable it can be taken as further evidence for the location of the sanctuary of Heros Iatros in Athens, in the north of the city, apparently a little within the Acharnian Gate: if not acceptable, it can at least be taken to suggest the existence in this quarter of the city of a hero sanctuary in which such an offering would be appropriate.

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<sup>82</sup> Kutsch, *op. cit.* 2 ff., Nilsson, *op. cit.* 538 f., Hausmann, *op. cit.* 23 f.

<sup>83</sup> Kutsch, *op. cit.* 7 f.



## THE CAVALRY BATTLE AT THE HYDASPES<sup>1</sup>

ALEXANDER's battle against the Indian king, Porus, at the R. Hydaspes is in many respects the most interesting of all his set engagements. There he had to cope with a new weapon, the elephant, and few things show his military genius more clearly than the resource which he displayed in solving this difficult problem. The main outlines of the battle are clear, but, despite all that has been written, there is little agreement amongst historians<sup>2</sup> regarding Alexander's tactics in the cavalry engagement which led up to the main action. The most recent treatment of the problem is that by Sir William Tarn,<sup>3</sup> and it is this which has led me to examine the question more closely. Although, as will become clear, I find myself at variance with Tarn's interpretation on numerous points he has, I believe, shown that it is only by a detailed study of Arrian's text that we can arrive at a satisfactory understanding of the orders to Coenus—the hub of the matter. I intend, therefore, to re-examine Arrian's narrative and to attempt to substantiate a view of the overall strategy—particularly the part played by Coenus—radically different from that put forward by Tarn. Moreover, Tarn has subjected Arrian's account of the battle and its preliminaries to such severe criticism that it is not, I think, unfair to say that, if Tarn's interpretation of the operations is accepted, Arrian must be held to have misunderstood his source on several important points and to have failed to make Alexander's tactics intelligible. This article has, therefore, a second purpose—to demonstrate that the undoubted difficulties in Arrian's narrative, due in part to the complicated manoeuvres involved in part to the ambiguity of military terminology, do not indicate any fundamental misunderstanding of the situation by the historian.

The battle is described in all our extant Alexander-historians,<sup>4</sup> but Tarn is in my opinion clearly right in treating Arrian as the only reliable source for the cavalry battle and the events immediately preceding. Plutarch, even if the letter of Alexander on which he bases his account is genuine,<sup>5</sup> treats the battle itself in too summary a fashion to be of much value. Curtius and Diodorus, though writing at greater length, are confused and unreliable and provide few details of importance. Arrian, then, must be the basis of our reconstruction. His account is clearly based on Ptolemy, who was in close attendance on Alexander<sup>6</sup> and whose authority on military matters needs no underlining. I see no reason to believe that Arrian's narrative reveals any other source for the actual engagement or that the difficulties in it are due to his attempting to combine divergent accounts.<sup>7</sup>

Alexander is said by Arrian<sup>8</sup> to have brought across the river some 6,000 infantry and 5,000 cavalry, but Tarn has shown that the figure for the infantry must be considerably increased, perhaps to 9,000 or 10,000. He has also suggested that the forces stationed along the bank of the river under Meleager, Attalus, and Gorgias were to cross as Alexander passed their positions on his way downstream. If this is so, and it is hard to see what purpose these troops could fulfil by remaining in their positions, then Alexander will have had with him a force of about 15,000 infantry.<sup>9</sup> This would explain how his troops were able to encircle the Indians in the later stages of the battle. If Tarn is right about this it means further that Alexander had more cavalry with him in the battle. For with the three commanders mentioned above were left the mercenary infantry and cavalry, and these mercenary forces must be supposed to have crossed as well as the regiments of the phalanx. We

<sup>1</sup> I am most grateful to Mr. G. T. Griffith for reading an early draft of this article and for making several valuable suggestions.

<sup>2</sup> For a full bibliography see G. Glotz and R. Cohen, *Histoire Grecque*, vol. iv. i. (2nd ed. 1945), p. 117 and p. 148 n. 156. The works of Brelocr and Kornemann listed there have not been accessible to me. V. A. Smith, *The Early History of India* (1924), pp. 65 ff. (followed by Glotz-Cohen), and C. A. Robinson Jr., *Alexander the Great* (1947) give a picture of the operations similar to mine, but do not discuss the sources in detail. For a plan of the battle see Smith, *op. cit.*, p. 82. There is, however, no warrant for placing a line of infantry in front of the elephants or for making the Indian line extend up to the river on the left. The arrangement of the elephants in eight ranks is not supported by the sources, but is due to Smith's views on the site of the battle.

<sup>3</sup> *Alexander the Great* (1948) vol. 2 app. 6 'The Battle of the Hydaspes', pp. 190 ff. (esp. pp. 193-6). A. R. Burn has remarked (*JHS* 1947, p. 141) that 'every serious student of Alexander, probably for generations, will have to start from Tarn's analysis of the sources and discussion of the chief problems of the narrative'. This judgement, with which I entirely agree, may perhaps justify my approach to the problem. R. Meiggs, in the third edition of *Bury's History of Greece* (n. to pp. 803-6) has already accepted Tarn's interpretation of Alexander's tactics.

<sup>4</sup> References: Arrian, *Anabasis* 5.11-18; Plutarch 'Alex-

ander' ch. 60; Diodorus Bk. 17.87-8; Curtius Bk. 8.13-14. The battle is also described by Polyæmus 4.3.22.

<sup>5</sup> There is no decisive evidence about this. Tarn himself admits (p. 197) that 'the earlier part of the letter . . . has been carefully done from good sources, and would pass muster' (cf. F. Schachermeyr, *Alexander der Grosse*, p. 520 n. 228). He is wrong to reject the letter because of the (alleged) difference between τῷ δεξιῷ and ὡς ἐπὶ τὸ δεξιόν (post). The sentence αὐτὸς μὲν ἐνέεισι κατὰ θάλασσαν κίρας, κοῖνον δὲ τῷ δεξιῷ προσβαλεῖν καλεῖσθαι does, in fact, agree with Arrian's account of the battle, although this does not, of course, prove that the letter is genuine.

<sup>6</sup> Arrian 5.13.1. αὐτὸς δὲ (sc. ὁ Ἀλκιμάδης) ἑπὶ τῷ τριαντάτῳ ἑπὶ καὶ ἅμα αὐτῷ Πτολεμαῖος τε καὶ Περδίκκας καὶ Λυσίμαχος αἱ σωματοφύλακες.

<sup>7</sup> For an exhaustive discussion of the relative merits of the ancient sources see G. Veith, *Die Kavalleriekampf am Hydaspes*, *Klio* 8 (1908), pp. 131 ff. Veith proves conclusively, I believe, that any reconstruction of the battle must be based primarily on Arrian, and effectively disposes of the attempt by R. Schubert (*Rh. Mus.* 56 (1901), pp. 543 ff.) to set aside Arrian's account in favour of Polyæmus. The main defect of Veith's article is that he does not support his conclusions (with which I am, in the main, in agreement) by reference to Arrian's own usage.

<sup>8</sup> 5.14.1 (cf. 51.8.3).

<sup>9</sup> It is interesting to note that Thirlwall had already doubted the number 6,000 and had put Alexander's forces as high as 20,000 men.



have no knowledge of the numbers of the mercenary cavalry at this time, but, if we hold that these contingents crossed, then Alexander will have had considerably more than 5,000 cavalry with him.<sup>10</sup>

After crossing the river about 150 stades upstream from his main camp (where he had left Craterus with a strong force) Alexander marshalled his forces in battle order. He then proceeded southwards with his cavalry, leaving the infantry to follow at a foot pace. Meanwhile Porus had sent out his son with a force of 2,000 cavalry and 120 chariots (if we accept Ptolemy's figures) either to reconnoitre or to oppose Alexander's crossing. In the event Alexander met this Indian force in his advance and defeated it with heavy losses, Porus' son falling in the battle. The survivors fled back to Porus with the news that Alexander had crossed and was advancing in full force. Thereupon the Indian king left a few elephants and a small detachment of troops opposite the Macedonian camp to prevent Craterus from crossing in his rear, and advanced northwards with the remainder of his forces. On reaching a sandy and level position he halted and drew up his troops in line of battle to await Alexander. His arrangements are clearly described by Arrian (15.5-7). In the centre in front he placed 200 elephants at intervals of 100 feet (διέχοντα ἐλέφαντα ἐλέφαντος οὐ μείον πλῆθους), while covering these intervals but on a second line behind the elephants he stationed the bulk of his infantry, totalling 30,000 in all. The remainder he drew up on either side of the beasts, and on the wings his cavalry, 2,000 on each side. In front of these were placed 300 chariots, presumably divided equally between the wings.<sup>11</sup> These dispositions were complete before Alexander decided on his order of battle, which was, indeed, determined by the presence of the elephants in the Indian centre. He could not approach these with his cavalry or advance by the centre with his infantry unless he were prepared to suffer heavy losses.

When Alexander observed that the Indians were forming up into line of battle he halted his cavalry to allow his infantry to come up. When they had done so he very wisely did not lead them forward immediately; instead he ordered his cavalry to circle around to give the infantry time to recover from their march. During this period the Indians completed their dispositions, and Alexander, observing the elephants in the centre, decided first of all to put the enemy cavalry out of action. Accordingly, says Arrian, he took with him the bulk of the cavalry and began to ride along against the enemy left, intending to make his attack there ἀλλ' αὐτὸς μὲν, ἅτε ἵπποκρατῶν, τὴν πολλὴν τῆς ἵππου ἀναλαβὼν ἐπὶ τὸ εὐώνυμον κέρας τῶν πολεμίων παρήλαυνεν, ὡς ταύτῃ ἐπιτησόμενος.<sup>12</sup> It should be noticed that his decision to attack on the left and the orders to Coenus and the infantry commanders (which must have been given before he rode off, although they follow in Arrian's text) occurred before he came within range. I mention this because, although there are no absolute times in the narrative, we can place the events relative to one another.

We come now to the orders to Coenus—the crucial point. Κοῖνον δὲ πέμπει ὡς ἐπὶ τὸ δεξιόν, τὴν Δημητρίου καὶ τὴν αὐτοῦ ἔχοντα ἵππαρχίαν, κελεύσας, ἐπειδὴ τὸ κατὰ σφᾶς στίφος τῶν ἱππέων ἰδόντες οἱ βάρβαροι ἀντιπαριππεύουσιν, αὐτὸν κατόπιν ἔχεσθαι αὐτῶν. The opening words Κοῖνον δὲ πέμπει ὡς ἐπὶ τὸ δεξιόν have been interpreted in one of several ways—Coenus was sent: (1) to Alexander's right; (2) towards the Indian right as a feint; (3) against the Indian right (to attack it). The first interpretation, although held by Bury and many other good scholars, can be dismissed quite summarily. It will be sufficient to quote Tarn (p. 196 n. 1): 'Porus' right, not Alexander's, proved by the reference just before to Porus' left.' This seems quite conclusive. The second view, first put forward by Bauer, is restated by Tarn. He translates 'as if (he were going) towards Porus' right', and remarks that ὡς ἐπὶ τὸ δεξιόν cannot possibly mean the same as τῷ δεξιῷ προσβολεῖν (in the letter quoted by Plutarch). If Tarn means that ὡς must be translated 'as if' he is quite evidently wrong. In Classical Greek ὡς with a preposition would normally indicate intention, and in Arrian this use is extremely common. I give two examples<sup>13</sup> relating to

<sup>10</sup> On the mercenary cavalry see H. Berve, *Das Alexanderreich*, vol. 1, pp. 148-9.

<sup>11</sup> If the elephants were drawn up in a single line (as Arrian seems to imply) the Indian line must have extended for about five miles across the plain. If we take the calculations of Polybius (12.20-1) as a basis, the Indian infantry, occupying six feet per man, cannot have been drawn up more than eight deep. One would expect a greater depth, and Veith (p. 139) may well be right in thinking that Porus' line did not extend so far. Veith rightly emphasises the difficulty of estimating the length of a battle-line even in modern times. Mr. A. R. Burn draws my attention to Plut. Alex. 62, 1, where Porus' forces in the battle are given as 20,000 infantry and 2,000 cavalry, and suggests that Arrian's figures may include all Porus' forces, and not merely those in the battle-line. The modest figures given by Plutarch (? from Alexander's letter) contrast sharply with the gigantic totals of the Indian troops across the Ganges mentioned in the same chapter, and are thus all the more credible. If they are correct it is even more doubtful whether the Indian line extended for five miles. There are many possible sources of the error, if error there is; Arrian may have failed to make it clear that the elephants occupied more than a single line or, perhaps more probably, the estimate of about 100 feet between the elephants may be an over-estimate.

Very probably Arrian has given us the information he found in Ptolemy, and it is the latter, if anyone, that is at fault.

<sup>12</sup> Tarn (p. 195 n. 3) translates 'he began to ride towards the Indian left, as if he was going to charge it (but he was not)'; adding, 'ὡς is common enough in this sense'.

I agree that the imperfect must have its proper force, i.e. 'he began to ride', but although ὡς may mean 'as if' there is nothing in the phrase itself to prevent our taking it as a statement of Alexander's real intentions, i.e. that he intended to attack the left wing eventually. I do not mean that he *now* galloped off to make his attack. The interpretation of the phrase, in fact, depends upon our view of Alexander's tactics (see also note 25).

<sup>13</sup> There are approximately ninety examples of ὡς ἐπὶ in Arrian. Apart from its use in geographical expressions meaning 'towards', as in Strabo, (see esp. Arrian's description of the rivers of Asia in Chapters 5 and 6 of Book 5), it is commonly used with the following verbs: ὄντω (and its compounds), προχωρεῖν, ἵκναι, and θαλάσσειν. Alexander's charge at Gaugamela (3.14.2) is described in these words: ἦγε δρόμῳ τε καὶ ἀλαλαγμῷ ὡς ἐπὶ αὐτὸν Δαρδάνου, while closely parallel to the present passage is 3.15.1 (also at Gaugamela) ὡς ἐπὶ τὸ δεξιὸν τῶν βαρβάρων ἦγε δρόμῳ (sc. ὁ Ἀλέξανδρος).



the movements before the battle—5.13.1 οἱ σκοποὶ κατιδόντες αὐτῶν τὴν ὁρμὴν ὡς ἐκάστοις τάχους οἱ ἵπποι εἶχον ἡλαυνον ὡς ἐπὶ τὸν Πῶρον—the scouts rode off to Porus; and 5.15.4 αὐτὸς δὲ (sc. ὁ Πῶρος) τὴν τε ἵππον ἀναλαβὼν πᾶσαν — — — ἡλαυνεν ὡς ἐπ' Ἀλέξανδρον—advanced against Alexander. In fact, Arrian used ἐπὶ and ὡς ἐπὶ interchangeably, e.g. 4.4.7. ἐπὶ γὰρ ἐπὶ τοὺς Σκύθας and 3.17.4. ἐπὶ γὰρ ὡς ἐπὶ τοὺς βαρβάρους, where there is no observable difference in meaning. On the other hand, there can be in Arrian few examples (I have noticed none) of ὡς ἐπὶ used in Tarn's sense. In view of this I would maintain that 'against the (Indian) right' is in itself by far the more probable rendering.

But if so how is the remainder of the order to be translated? Tarn takes this to mean 'when the Indian cavalry should see the body of cavalry facing themselves, κατὰ σφᾶς (σφᾶς is the Indian cavalry), and should attack it, Coenus was to swing round (to his own right) and take them in rear.' But if this is correct a difficulty arises. For the βαρβαροὶ are clearly those on the Indian right, while σφᾶς, if we take this to refer to the Indians, can only mean those on the left, i.e. opposite Alexander. This is certainly awkward and not free from ambiguity. The difficulty does not arise for Tarn, since he assumes that before Coenus received his orders all the Indian cavalry were concentrated on the left, i.e. that the right wing had already ridden along to support their left. Arrian, however, clearly implies that the orders were given first, and Tarn himself admits as much. With reference to 17.1 ἐν τούτῳ δὲ οἱ τε Ἰνδοὶ τοὺς ἱππέας πάντοθεν ξυναλίσσαντες he writes (p. 195): 'one can hardly say that Arrian is wrong, for ἐν τούτῳ might mean anything; but it hardly suggests, as must have been the case, that Porus had already massed all his cavalry on his left before Alexander gave his orders.' Indeed, unless we are to stretch ἐν τούτῳ to cover a very long period of time, Arrian certainly believed that the orders were given before the transfer took place. Tarn's view, then, demands that we reject the testimony of Arrian on this point. Of course, it also demands that we take ὡς ἐπὶ τὸ δεξιόν in his sense—that Coenus rides only so far towards the Indian right that he can wheel round in the rear of the Indians as they advance against Alexander. We must, therefore, decline to accept Tarn's interpretation here.

It is worth while considering whether σφᾶς may refer not to the Indians but to Alexander and whether in this case κατὰ may not mean 'beside, near' instead of 'opposite, facing'. On this view σφᾶς will refer to the subject of the main verb (Alexander), i.e. it will be an indirect reflexive representing ἑμᾶς (or ἐμέ) of the direct speech. Now it is true that σφᾶς is regularly used by Arrian as a direct reflexive<sup>14</sup> in contrast to the normal Attic usage; but despite the lack of reported speech in Arrian (a fact to be borne in mind) there are a few instances where he uses it as in Attic. The most striking example occurs at 5.11.4, where Alexander gives Craterus his orders before the crossing—a passage worth quoting in full: παρηγγέλλετο δὲ Κρατέρῳ μὴ πρὶν διαβαίνειν τὸν πόρον πρὶν ἀπαλλαγῆναι Πῶρον ξὺν τῇ δυνάμει ὡς ἐπὶ σφᾶς ἢ φεύγοντα μαθεῖν αὐτοὺς δὲ νικῶντας. ἦν δὲ μέρος μὲν τὴν στρατιᾶς ἀναλαβὼν Πῶρος ἐπ' ἐμέ ἄγῃ, μέρος δὲ τὴν ὑπολειφθεῖσα αὐτῷ ἐπὶ στρατοπέδου καὶ ἐλέφαντες, οὗ δὲ δὴ καὶ ὡς μένειν κατὰ χώραν. εἰ δὲ τοὺς ἐλέφαντας ζυμπαντας ἅμα οἱ ἄγῃ Πῶρος ἐπ' ἐμέ, τῆς δὲ ἄλλης στρατιᾶς ὑπολείποιτό τι ἐπὶ στρατοπέδου, οὗ δὲ διαβαίνειν σπουδῇ. οἱ γὰρ ἐλέφαντες μόνοι, ἔφη, ἀποροὶ εἰσι πρὸς τοὺς ἐκβαίνοντας ἵππους. ἡ δὲ ἄλλη στρατιὰ εὐπορος. Obviously ἐπὶ σφᾶς in the indirect speech and ἐπ' ἐμέ in the direct both refer to Alexander, and I would suggest that this passage affords very strong support for the view put forward above. Moreover, at Gaugamela, when hard pressed by the encircling Persian troops, Parmenio sends to Alexander a messenger ἀγγελοῦντα ὅτι ἐν ἀγῶνι ξυνέχεται τὸ κατὰ σφᾶς καὶ βοηθεῖν δεῖ.<sup>15</sup> Here again σφᾶς is used to refer to the subject of the main verb. The use of κατὰ with the accusative to mean 'beside, near', although not noticed by Liddell and Scott (and although much less common in Arrian and the historians generally than that meaning 'opposite'), undoubtedly does occur. I have given one instance of this use at 3.15.1 (above) and 2.10.3 ὡς δὲ ἐντὸς βέλους ἐγίγνοντο, πρῶτοι δὲ οἱ κατ' Ἀλέξανδρον καὶ αὐτὸς Ἀλέξανδρος — — — δρόμῳ ἐς τὸν ποταμὸν ἐνέβαλον puts the matter beyond doubt.<sup>16</sup>

Apart from the question of ὡς ἐπὶ, Tarn's interpretation has other disadvantages. It necessitates our taking στίφος in the sense of a small body of cavalry, although, as he himself points out, this word has already (15.2) been used of the whole body of horse under Alexander. It may not be impossible to take στίφος in this sense, but it seems far from likely. Again ἐχεσθαι would more naturally mean 'follow closely' than 'take in the rear', although here again it cannot be said that the latter is altogether impossible.<sup>17</sup> ἀντιπαριπτεῖν occurs only in this passage, and the meaning must therefore be determined by the context. I suggest that 'rides along to meet' is at least as

<sup>14</sup> See Abicht's edition at 4.16.2.

<sup>15</sup> Arrian 3.15.1. Cf. also 3.2.3. and 4.29.1.

<sup>16</sup> See also 3.9.6 and 5.23.7. This use of κατὰ is not uncommon in Plutarch (cf. *Nicias* 18.3 and *Them.* 29.1), and *Alex.* 32 affords an excellent example—τοὺς γὰρ ὁ ἀγὼν ὑποτροπήν καὶ σάλον ἐν τῷ εὐανίῳ κέραι κατὰ Παρμενίωνα, τῆς Βακτριανῆς ἵππου ῥόφῳ πολλῶν καὶ μετὰ βίας παρεμπεισοῦσης εἰς τοὺς Μακεδόνας—where the left wing is the Macedonian, not the Persian. If we compare the use in Polybius (26.7) of τὰ κατὰ τοὺς Ῥόδιους as the equivalent of οἱ Ῥόδιοι with the common

Hellenistic use of οἱ περὶ πᾶσα as a periphrasis for the person himself (cf. Strachan-Davidson, *Selections from Polybius*, pp. 114–15) we can see that in later Greek there is little difference between κατὰ and περὶ in this sense. In the passage in Arrian κατὰ is virtually equivalent to περὶ (or ἀμφί).

<sup>17</sup> LS<sup>9</sup> s.v. ἐχω c. 2 cite only Demosthenes 18.79 for ἐχεσθαι 'fasten upon, attack', but W. W. Goodwin in his edition of the *De Corona* rightly renders 'clung to, followed up closely'. For this use in Arrian see 4.5.8. and 4.6.5.



likely a translation as 'attack'.<sup>18</sup> Certainly *παριππεύειν* frequently means 'to ride along' in Arrian, cf. e.g. 5.22.5, and esp. 2.9.1.

One last point—Tarn remarks that 'the point of the order is the word *ιδόντες*'. I do not feel convinced of this, but, if the word is to be stressed the meaning will be 'when Porus realises (after Coenus' departure) that Alexander's main cavalry force is concentrated on his left'. I should therefore take the orders to mean that 'when the Indians, seeing the (large) body of cavalry with (beside) Alexander, should ride along to meet it, Coenus was to follow closely in their rear'.

Three questions must now be answered: 'What caused Porus to order the cavalry on the right to ride round to the left?'; 'Did the Indian cavalry (and presumably Coenus) ride along the front of their line or behind it?'; and lastly, and most important, 'How could Alexander *know*, when he gave Coenus his orders, that the transfer would take place?'

The first question has been partly answered already. It was the presence of a large body of cavalry opposite his left that determined the Indian king to order the transfer. After Coenus had been sent to his left Alexander had under his command at least 3,000 cavalry—at least since, if the mercenary cavalry crossed the river after Alexander passed their positions, he may have had many more. But even if we reject this hypothesis (which perhaps cannot be proved) Alexander outnumbered the Indian left by at least three to two. Wilcken<sup>19</sup> suggested that it was Alexander's charge that brought about Porus' action, but this can hardly be accepted. The danger must have been apparent some time before Alexander actually charged, while if it was the charge which provided the motive for the transfer it is hard to see how the cavalry could have reached the left in time to meet it, as is required by Arrian's narrative (17.1). It was rather the *threat* of the charge than the charge itself which decided Porus.

Veith (p. 137) holds that the Indian cavalry passed in front of their own line, i.e. in front of the elephants, for three reasons: (1) the transfer would be quicker; (2) the Indians could attack Alexander in flank; (3) after the initial engagement in front of the line the Indians withdrew behind the elephants. The first, though correct, is not important, since the difference in time would be slight. The second, I think, affords no support for his view. Certainly the Indians did not succeed in attacking Alexander in flank and, as we have seen, he had not yet begun to make his attack when Porus gave his order. It is, of course, true that his attack was expected. A more weighty objection is that the Indians themselves, during their move, would be exposed to a flank attack by Coenus. With regard to his third reason, I cannot agree that Arrian's text supports the view that the cavalry engagement took place in front of the elephants. What Arrian says is *ἀλλὰ κατηράχθησαν* (sc. οἱ Ἰνδοί) *ὥσπερ εἰς τεῖχος τι φίλιον τοὺς ἐλέφαντας*, which simply means that the Indians withdrew to the shelter of the elephants. Moreover, there is good reason to believe that the initial clash took place to the left of the Indian line and not in front of it (post). But the overriding factor is that untrained horses will not approach elephants, as Arrian frequently remarks and Tarn very properly insists. Certainly Coenus could not have passed in front of them, and it seems most likely that he followed the Indian troops. This is quite certain if, in the orders to Coenus, Arrian has reproduced what actually took place. I shall come to this. On balance, I think it much more probable, though perhaps not yet proved, that both groups of horse passed *behind* the Indian lines.

But how could Alexander *know* that Porus would concentrate all his cavalry on the left? It is one of the merits of Tarn's interpretation that this question need not be asked; when the orders were given the cavalry were already there. But this view involves, as we have seen, the rejection of Arrian's narrative, and before resorting to this we should consider whether any alternative explanation can be found. It is usual to say that Alexander possessed the ability to foresee the course of events, but this means no more than that Alexander was prepared to take a chance, to gamble on Porus' reactions. Now every general must on occasion take risks, and Alexander was no conservative in this respect. But was it necessary in this case to detach Coenus, in order that he might ride round and fall upon the rear of the Indians when they had concentrated upon the left (or rather just as they were concentrating)? If this was Alexander's plan it was dangerous, not so say rash, and required a very nice sense of timing. It is perhaps questionable in view of the distances involved whether such a plan could be more than a gamble. The suggestion has, however, been made to me by Mr. G. T. Griffith that Arrian is here at fault, that he has been guilty of omission in that he has given us only a portion of the orders to Coenus.<sup>20</sup> It will, of course, be asked what evidence there is for this hypothesis. Definite proof cannot in the nature of things be expected, but if we turn to Alexander's orders to Craterus (given above) we see that they are extremely detailed and cover a wide range of possibilities. It seems not improbable that the orders to Coenus were

<sup>18</sup> I now find that J. W. McCrindle, *The Invasion of India by Alexander the Great*, p. 104, takes it thus.

<sup>19</sup> *Alexander der Grosse*, p. 171 (E.T., p. 182). Tarn rejects this suggestion because of his conviction that Alexander did not charge, but, by sending away Coenus, induced the Indian cavalry (4,000 strong) to charge him, and thus allow Coenus to take them in the rear.

<sup>20</sup> It is, of course, possible that the fault is not Arrian's but Ptolemy's; that Arrian has reproduced all the information available to him. But Ptolemy, whatever else he knew about the battle, must have known, one would think, Alexander's orders to Coenus. Even if he described only his own 'acta', as Breloer holds, one would expect him to have dealt fully with the orders.



similarly detailed. It cannot have made any difference, one would think, to Alexander where the Indian cavalry were destroyed, whether on one wing or the other. The main thing was that they should be destroyed. If the worst came to the worst Coenus would surely be able to hold his own with the Indians until Alexander, after defeating the Indian left, should come to his aid. With his superiority in cavalry Alexander could hardly have had any doubts about his ability to defeat the left wing. His orders to Coenus may then have read roughly as follows: 'Proceed against the Indian right and destroy them; if they change their station and ride across to join Porus on the left wing, follow closely in their rear.' If so Arrian has given us only the alternative which actually occurred; had he written ἐάν instead of ἐπειδάν the whole matter would have been much clearer. The acceptance of this suggestion is not, I think, vital to my reconstruction, but it does clear Alexander of the charge of rashness to which he is otherwise exposed.

Now follow the orders to the infantry. They are to take no part in the action until they see the main body of the enemy infantry and cavalry (to the left of the elephants) thrown into confusion by his own cavalry force. The situation is now as follows—Coenus is moving forward to his left to attack the enemy right, while Alexander with his cavalry is proceeding to the right. Meanwhile Porus orders his cavalry on the right wing to ride along to support his out-numbered left. As his forces come within range (ἐντὸς βέλους) Alexander orders his horse-archers to throw the Indian left into confusion by discharging their arrows and by charging.<sup>21</sup> The movements of the cavalry under Alexander are then described (16.4) in a sentence which has been variously translated and is certainly not free from ambiguity: καὶ αὐτὸς δὲ τοὺς ἑταίρους ἔχων τοὺς ἵππεας παρήλαυνεν ὁξέως ἐπὶ τὸ εὐώνυμον τῶν βαρβάρων, κατὰ κέρας ἔτι τεταραγμένοις ἐμβαλεῖν σπουδὴν ποιούμενος, πρὶν ἐπὶ φάλαγγος ἑκταθῆναι αὐτοῖς τὴν ἵππον. The stumbling-block is κατὰ κέρας which can in itself equally well mean 'in column' or 'in flank', and in the former case can apply here either to the Indians or to the Macedonians. The repetition of παρα- in the words παρήλαυνεν,<sup>22</sup> παριπτεύων, and ἀντιπαρεξάγοντες suggests that Alexander was moving forward and towards the right in order to make a flank attack upon the Indians and that they were also moving outwards to counter this. Also in favour of taking κατὰ κέρας to mean 'in flank' is the very similar passage at 3.14.6: οἱ δ' ἐπὶ τοῦ δεξιοῦ κέρας τῶν Περσῶν ----- περιπτεύσαντες τὸ Ἀλεξάνδρου εὐώνυμον κατὰ κέρας τοῖς ἀμφὶ τὸν Παρμενίωνα ἐνέβαλον. In this passage it is quite certain that κατὰ κέρας has this meaning. I should therefore translate—'then Alexander himself began to ride rapidly along against the enemy left with the Companion cavalry, hastening to make a flank attack upon them while they were still in confusion, before their cavalry could form up in line'. Even if we take κατὰ κέρας to mean 'in column', I think it is quite clear that Alexander was bent on attacking the Indians in flank, although in this case his intention will not be specifically stated. It does seem to me, however (though this is subjective), that it is more likely that Arrian should mention Alexander's manoeuvre than the Indian formation, which can readily be inferred from the last part of the sentence.

I might have left the matter there had not Tarn maintained that Alexander did not (intend to) charge but, by sending Coenus off towards the Indian right, lured the Indian cavalry into attacking the (now outnumbered) force under his command while Coenus fell upon their rear. Now, even if we are prepared to grant that ὡς ἐπιθησόμενος (16.2) means *as if* he were going to charge, there can be no doubt that ἐμβαλεῖν σπουδὴν ποιούμενος does refer to Alexander's intention to attack. How, then, does Tarn dispose of this difficulty? Taking κατὰ κέρας to mean 'in column' and referring it to the Indian cavalry, he writes (p. 195): 'The words from κατὰ κέρας to the end of the sentence (p. 194 n. 4) are no part of Ptolemy's description [my italics]; the left wing was never in column, as here assumed, though the right was, on its way round to the left.' In the note just referred to Tarn supplies two further reasons for his view. 'παρελάυνει ἐπὶ', he writes, 'in the sense of "ride toward" is a common usage of Xenophon's (see Liddell and Scott s.v.); this, apart from the mistake of fact in applying κατὰ κέρας to Porus' left wing, shows that Arrian is not here transcribing Ptolemy but is writing in his own person. Another thing which shows this is that this is the only passage in which Arrian, instead of talking of the hipparchies, includes the Iranian cavalry under the term "Companion cavalry"; elsewhere they are distinguished, obviously because Ptolemy did so.' But his reasons for thinking that Arrian is here writing in his own person and not following Ptolemy are not, I think, convincing. The use of παρελάυνει in the sense of 'ride toward' is not common in Xenophon, as Tarn maintains, and if it were why should this mean that we have Arrian here, and not Ptolemy? <sup>23</sup> In the second place Tarn seems to mean that cavalry would be in column only

<sup>21</sup> In criticising Arrian's time sequence Tarn (p. 195 and n. 1) remarks that Coenus could not have been sent off before the horse-archers 'or he would have blocked them', and later adds (p. 196) that 'the Indians might suppose that he (Coenus) was going to support the horse-archers'. But the two units were clearly proceeding in different directions, and Coenus would be well clear to the left when the horse-archers charged.

<sup>22</sup> It is difficult to know exactly what meaning to assign to παρελάυνει, but the usual force of παρα in compounds of this kind is 'past' or 'along', and the latter seems to fit here.

Heitland (*Alexander in India*, p. 122) writes: 'Arrian (5.16.2) tells that Alexander was making a flanking movement (παρήλαυνεν) with the bulk of his cavalry to attack the enemy's left wing.' This expresses my view exactly, but I cannot agree with the remainder of his interpretation.

<sup>23</sup> LS<sup>9</sup> cite only two instances of παρελάυνει ἐπὶ in this sense. Tarn himself writes (p. 137): 'Finally it is worth remarking that Arrian wrote in the second century A.D., and that his use of common words is not invariably that of a Xenophon or a Demosthenes.'



when riding into position, not when drawn up in battle order. But is this true? Such evidence as I have found suggests that it is not. Onasander, who wrote a *λόγος στρατηγικός* under Claudius but obviously used material much older and who though dull is no fool, writes of the cavalry commander 'ταττέτω δ' ὡς τὰ πολλὰ κατὰ τὰς ἐκ παρατάξεως μάχας ἐπὶ κέρως', i.e. he should draw up his cavalry in column.<sup>24</sup> It is perhaps also relevant that the Macedonians at Gaugamela attack in a wedge formation which suggests rather a column than a line.<sup>25</sup> Even if *κατὰ κέρως*, then, does mean 'in column' (and I should prefer to take it otherwise), it is not, I think, proved that Arrian is wrong in referring this to the Indian left. As to Tarn's third point, it certainly seems that Arrian is writing loosely, but it is not a sufficient reason by itself to prove that Arrian is here writing in his own person and that Alexander did not in fact charge the Indian left. Now it follows that if Alexander charged the Indians there is no room for Coenus, if he rode along in front of the Indian line, to fall upon their rear. But that he did so is admitted by all. Alexander's charge, then, affords the final proof that Coenus and his men passed behind the Indian lines. The remainder of Arrian's account need not detain us. After the Indian cavalry had concentrated on the left Coenus appeared in their rear, according to orders. As part of the Indian force turned to meet Coenus Alexander perceived his opportunity and delivered his attack. The Indians fell back to the shelter of the elephants, which advanced against the Macedonian cavalry; the Macedonian phalanx moved up and the battle entered upon a new phase (17.3).

It remains only to summarise our conclusions. It has been shown, that Tarn's interpretation involves two important assumptions: that (contrary to Arrian) *all* the Indian cavalry were concentrated on the left before Coenus received his orders and that Arrian is wrong when he says that Alexander did eventually charge. There is, moreover, a further objection to this view. We must suppose in this case that Coenus moved only so far to the left that he could fall upon the rear of the Indian cavalry when they charged. But if so what was Porus to suppose that Coenus was doing? Certainly he could not be imagined to be proceeding to support the horse-archers. I find it hard to believe that Porus would have been deceived by so transparent a manoeuvre as is envisaged here. The wording of the orders to Coenus shows that he was sent to attack the Indian right and that both groups rode *behind* the Indian lines, not in front as Veith believed. Meanwhile Alexander rode forward to make his main attack on the Indian left. It is, I think, probable that after seeing the departure of the Indian cavalry from the right he timed this attack to coincide with the arrival of Coenus in the wake of the Indians, but it does not follow that when Coenus was dispatched Alexander counted upon the move. This reconstruction is based entirely upon Arrian and supported by reference to his own vocabulary and usage. There must always remain a certain difficulty in the reconstruction of battles in ancient times owing to the ambiguity of the terminology, but I would submit that my interpretation is supported by the parallels I have cited, and that there is no improbability in Arrian's account. Although it is true that Coenus had a long way to travel, especially if we accept the length of the Indian line as about five miles (and this may well be too long), the Indian cavalry also had to travel from one wing to the other. In fact, the extra distance which Coenus had to travel was only that from his point of departure to the Indian right, and by the time that the Indian move began he would be well on his way. There seems no reason why he could not appear on the Indian left shortly after the Indians, as Arrian says. If we are to criticise Arrian (or Ptolemy) it can only be because: (1) he has failed to state that the transfer took place behind the lines (perhaps he thought that his emphasis on the impossibility of untrained horses approaching elephants was sufficient); (2) he has, perhaps, given us only part of the orders to Coenus; and (3) he may be wrong about the length of the Indian line. None of these omissions or errors (and (2) and (3) are not certain) amounts to a misconception of the tactics involved, and Arrian's description of the battle should not lower our opinion of him as a military historian.

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<sup>24</sup> Sect. 16 (Loeb ed. p. 444). Xenophon 'The Cavalry Commander' II. 2-3 seems to imply that the Athenian cavalry regiment was organised in column.

<sup>25</sup> Arrian 3.14.2. Asclepiodotus 7.3 (Loeb ed. p. 279) also refers to the Macedonian wedge formation.



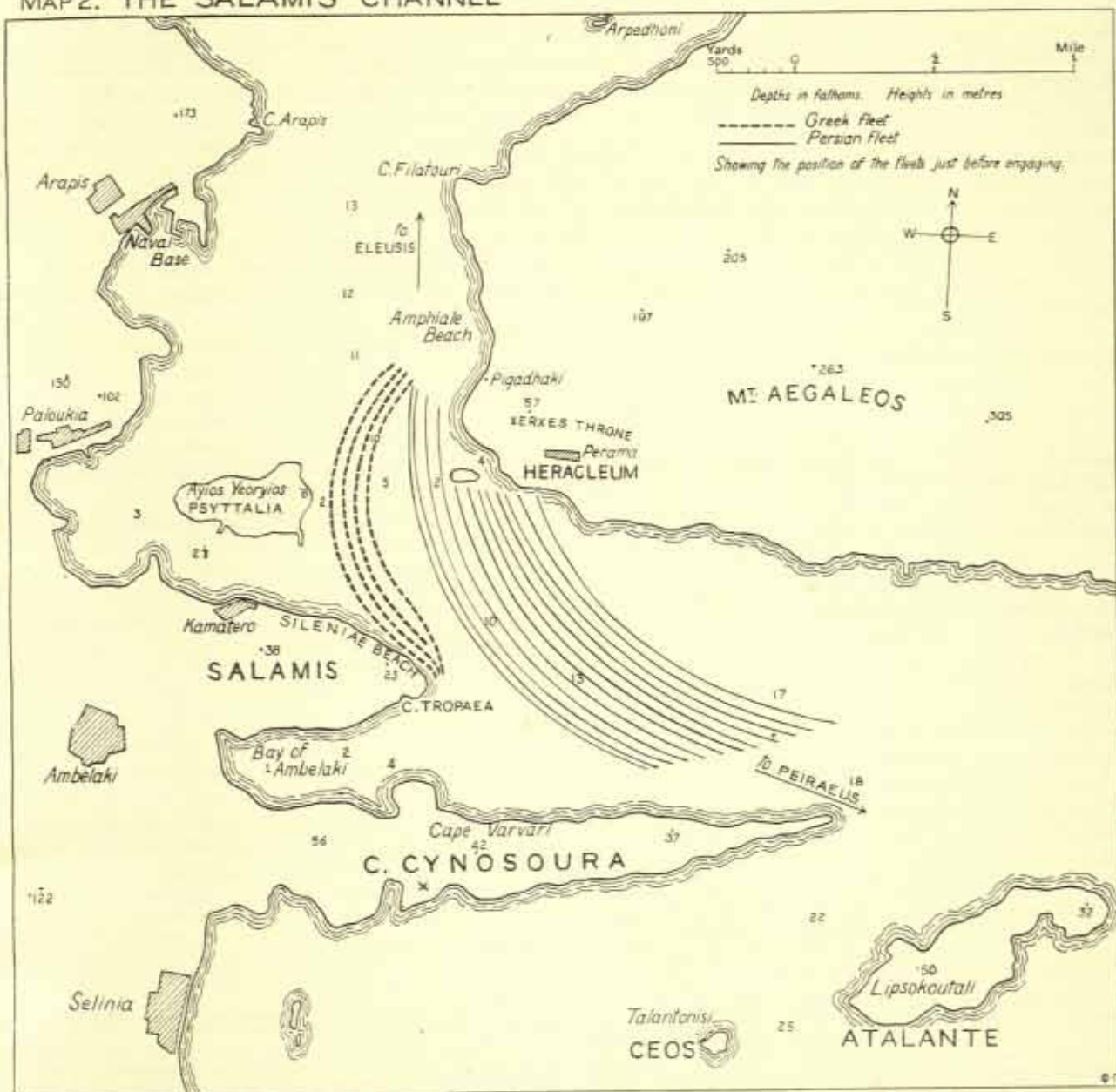
The maps and the geographical data are derived from the Greek Staff Maps 1/20,000 of the island; 1/100,000 of Greece; and for civil divisions on the recent map 1/100,000 of the Greek Statistical Division of the Staff; Admiralty Chart 894; and the *Mediterranean Pilot* 4 (1941) 107 f.



## I. THE TOPOGRAPHY OF THE SALAMIS CHANNEL

Several ancient authors mention topographical details of the Salamis Channel. These authors may be of varying value in matters in which personal judgment is involved; but, when they come to matters of topographical fact, one is no more dependable than another. For example, throughout antiquity the Heracleum, the island Psyttalia, or the mountain Aegaleos did not move. They were

MAP 2. THE SALAMIS CHANNEL



fixed points in the scene, which anyone could describe equally well. I propose, therefore, to accept the ancient evidences on matters of topography without demur.

These evidences, fortified in one case by excavation, give us a number of fixed points. Mt. Aegaleos is the long ridge between Mt. Parnes and the bend in the Salamis Channel; it is pierced by the pass which leads from Eleusis to Athens.<sup>1</sup> The ancient town of Salamis faces on to the Salamis Channel opposite Mt. Aegaleos (Hdt. 8.90.4, ὑπὸ τῷ ὄρει τῷ ἀντίον Σαλαμῖνος τὸ καλεῖται Αἰγάλεως),<sup>2</sup> and its remains have been uncovered on the peninsula known today as Kamatero. In the second century A.D. Pausanias visited the ancient town of Salamis (1.35.3-36.2). He

<sup>1</sup> Thuc. 2.19.2; Schol. Dem. 24.129; Aristodemus I 2 (FHG V), regarding Mt. Aegaleos as an extension of Mt. Parnes, placed Xerxes' throne ἐπὶ τοῦ Πάρνηθος ὄρους.

<sup>2</sup> Hdt. probably refers here to the town which is a much more accurate point of departure for the description than the island of Salamis; so, too, Diodorus 11.18.3, εἰς τὸν ἐναντίον τόπον τῆς Σαλαμῖνος.



mentioned the ruins of the Agora, the sanctuary of Artemis, the trophy of the Battle, the sanctuary of Cychreus, and the island called Psyttalia in front of Salamis (νησος δὲ πρὸ Σαλαμῖνος ἐστὶ καλουμένη Ψυττάλεια).<sup>3</sup> As Pausanias' point of reference was the town of Salamis, Psyttalia was the island in front of Kamateró called today Áyios Yeóryios; for no other island is in front of Kamateró. On this island, he continues, where a force was landed by Xerxes and destroyed by the Greeks, there were wooden idols of Pan. If any proof of the continuity of Greek life during the classical era is needed, Aeschylus *Persae* 447-9 affords it; for it was on this island in 480 B.C. that 'dance-loving Pan is wont to tread'. Pausanias refers again (4.36.6) to Ψυττάλειαν τὴν ἐπὶ Σαλαμῖνα, 'Psyttalia by Salamis', a phrase in which the preposition is more appropriate to the definite place, the town of Salamis, than to the island of Salamis as a whole. Stephanus Byzantinus s.v. *Atalante*, having mentioned the town and the island of that name off Locris, continues καὶ ἄλλη πρὸς τῷ Πειραιεὶ ὁ νησιώτης Ἀταλάνταιος.<sup>4</sup> There are two islands 'off Peiraeus', namely Lipsokoutáli a mile or so long, which is now inhabited by some personnel of the Greek Navy, and Talantónisi, a mere rock which is awash in a rough sea and cannot have been inhabited. Of these two islands Lipsokoutáli alone meets the requirements of Stephanus; for the ethnic form which he gives means that the island was inhabited. Atalante, then, is the island called today Lipsokoutáli. Nor can anyone who has sailed from the Saronic Gulf into the Peiraeus seriously doubt the identification.

Because the descriptions which we have cited are factual, unambiguous, and dependable, they yield four footholds to the topographer: the mountain Aegaleos, the town Salamis and the islands of Psyttalia and Atalante. These footholds are of value when we turn to Strabo's consecutive account of the Salamis Channel and its approaches (Str. 9.1.13-14, C 395).

Strabo's viewpoint is that of a man sailing through the Bay of Eleusis, the Channel of Salamis, and the open waters of the Peiraeus. His stages are marked by εἴτα, and he begins as follows: εἴτ' Ἐλευσίς πόλις . . . . εἴτα τὸ Θριάσιον πεδῖον καὶ ὁμώνυμος αἰγιαλὸς καὶ δῆμος· εἴθ' ἡ ἄκρα ἢ Ἀμφιάλη καὶ τὸ ὑπερκείμενον λατόμιον, καὶ ὁ εἰς Σαλαμίνα πορθμὸς ὅσον διστάδιον, ὃν διαχοῦν ἐπειρᾶτο Ζέρξης, ἔφθ' δὲ ἡ ναυμαχία γενομένη καὶ φυγὴ τῶν Περσῶν. The termini are clear enough, namely Eleusis and the bend of the Salamis Channel; for the passage from the Attic coast to the town of Salamis, or for that matter to the island of Salamis, is made from Péráma. It is true that the distance from Péráma to Kamateró is the same as that from Cape Filatourí to Cape Arápis, but Péráma is closer to Athens, and the centre of ancient habitation on the island was at Kamateró. Moreover, Strabo's note on Xerxes' mole is clarified by the statements of three authors; Ctesias fr. 26, ὁ δὲ Ζέρξης αὐτόθεν ἐλθὼν ἐπὶ στενιότατον τῆς Ἀττικῆς—Ἡράκλειον καλεῖται—ἐχώννυε χῶμα ἐπὶ Σαλαμίνα, Aristodemus I 2 (*FHG* 5.1) ἐσπούδασε δὲ ὁ Ζέρξης ζεύγμα κατασκευάσας πεζῇ ἐπιβῆναι ἐπὶ τὴν Σαλαμίνα . . . καὶ μέρος τι ἔχων ἦκεν κατὰ τὸ Ἡράκλειον, and Phanodemus in Plutarch *Themistocles* 13, describing the position of Xerxes' throne, ὑπὲρ τὸ Ἡράκλειον ἢ βραχεῖ πόρῳ διείργεται τῆς Ἀττικῆς ἡ νῆσος. When I asked a Greek sailor<sup>5</sup> on which line one would build a mole from Attica to Salamis, he replied at once 'from Péráma to Áyios Yeóryios and from Áyios Yeóryios to Kamateró'. This line has two advantages over any other line; it is shorter, and the water is shallower. Between Áyios Yeóryios and the rock off Péráma the channel was deepened in recent years to permit the passage of the Greek cruiser 'the Avérof'. Here the deepest point is now c. 30 feet, whereas the depth between Cape Arápis and Cape Filatourí attains 70 feet. There is, then, no doubt that Strabo, Ctesias, Aristodemus, and Phanodemus had in mind the narrows between Péráma and Kamateró. The Heracleum, therefore, was at or near Péráma. The Cape Amphiale, lying between the Thriasian coast and Péráma, may be identified with Cape Filatourí. The quarry which existed in Strabo's day was between Cape Filatourí and Péráma, an area which is now barred to visitors by the Greek naval authorities.<sup>6</sup>

<sup>3</sup> The sanctuary of Artemis was probably on the hill of Kamateró, where a decree of her thiasotae was found (Milchhöfer 7.27; *IG* 2.620). The trophy, with the cult of Zeus Tropaíos, was on a peninsula (Schol. A. *Pers.* 303, Σιληνία αἰγιαλὸς ἐστὶ τῆς Σαλαμῖνος τῆς λεγομένης Τροπαίας ἄκρας), which may be identified with Cape Kamateró (see Appendix). Milchhöfer and others prefer Cape Varvári and suggest that the cape had two names, Cynosoura and Tropaia. The Polyandreon, which is known from the inscription first published in *Ephem. Archaeologiké* 1884, p. 169, was the tomb of the Greek dead; its site is not known, but it was probably not far from the trophy. There is no doubt that Kamateró is the site of classical Salamis town; when Strabo mentions an early Salamis town which faced south towards Aegina (9.1.9, C 393), he is presumably speaking of the pre-classical or 'Homeric' town.

<sup>4</sup> For καὶ ἄλλη meaning 'there is another Atalante', compare Steph. Byz. s.v. *Oropos*.

<sup>5</sup> Mr. V. Deleyannis, mentioned in n. 1. Cf. Goodwin 257 'the only passage over which it would not have seemed insane even for Xerxes to attempt to build a causeway to Salamis is from this point of Aegaleos (a little north-west of the ferry), over the shoal above mentioned, to the island of St. George and thence to Salamis'; despite this, Munro, *CAH* 4, 395, thought

that Xerxes intended to build a causeway to the tip of Cape Varvári.

<sup>6</sup> Leake, *Die Deme von Attika* 159, mentions two possible sites for the quarry, one on the peninsula of Skaramangá, where there is an artificial mound of excavated soil, and the other on the coast east of the reef of Arpedhóni, where he speaks of a 'small ancient quarry'. Leake evidently preferred the former site for Strabo's quarry. Lolling, p. 6, mentions a quarry near Péráma and places it on the hill just north-west of Péráma on his map; Lolling claims it is the only quarry between Péráma and Skaramangá, and denies Leake's two sites without justification. It is clear that a suitable site is available whether Cape Amphiale is equated with Cape Filatourí or with the bend of the Channel by Péráma which significantly is not named as a cape on modern Greek maps, and the precise dating of a quarry is too difficult to permit one to say that one and not another of the three sites was quarried in Strabo's day. As regards Heracleum, Lolling recognises that for the purposes of the mole, Heracleum must be at or near Péráma, but he wishes also to locate a Heracleum at Keratsini Bay; his argument that the former term is used 'in a wider sense' and the latter 'in a narrower sense' would be acceptable only if Heracleum was a deme and not a shrine.



At this point we must digress to consider Strabo's statement ὁ εἰς Σαλαμίνα πορθμός ὅσον διστάδιος. One view is that the passage is corrupt, and another is that Strabo has made a gross miscalculation; for 'about two stades' is more or less 400 yards, and the distance from Péráma to Kamateró is some 1,200 yards.<sup>7</sup> A third view is that Strabo referred to the narrowest stretch of water in the Channel, namely from the rock off Péráma to Áyios Yeóryios, which measures today some 800 yards.<sup>8</sup> This raises the difficult question whether the level of the sea is higher now than it was in antiquity. In the North Sea there is evidence that in the early centuries A.D. and again at about A.D. 1200-1300 the level of the sea rose and so attained its present height.<sup>9</sup> If this rise was general to all oceans, it must have taken place also in the Mediterranean Sea. Of such a rise there is evidence on the north-west coast of Greece, especially at Ambracius in the Gulf of Arta,<sup>10</sup> and in the Saronic Gulf in the case of the ship-sheds at the Peiraeus and at Sunium.<sup>11</sup> But the degree of the rise since antiquity is not certain. All one can say at present is that a total rise of some 5 or 6 feet, that is to say about 1 fathom, is probable.<sup>12</sup> If one assumes a rise of one fathom, then the following changes occur in the topography of the Salamis Channel. The open water between Áyios Yeóryios and the rock off Péráma becomes some 600 yards. Both Áyios Yeóryios and the rock off Péráma remain as islands, but both are more extensive. In the north of the Channel the island of Nerá almost ceases to be an island, being separated from Salamis by water only 1 foot or 2 feet deep. The two islands known as the Kyrádhēs almost join, for they become separated by water of much the same shallowness, but the eastern island is still cut off from the Attic coast and remains an island. The coasts are almost unaffected. In the southern part of the Channel the waters of the bay of Ambeláki become diminished by almost one-third, but the other coasts are hardly affected. Outside the Channel the island of Lipsokoutáli remains the same, being steep-to, and the rocky Talantónisi becomes very little larger than now.<sup>13</sup> So far as the Battle of Salamis is concerned, such a change makes no difference to the tactics of the fleets.

Strabo continues with his account rather vaguely as follows: ἐνταῦθα δὲ καὶ αἱ Φαρμακοῦσσαι, δύο νησία, ὧν ἐν τῷ μείζονι Κίρκης τάφος δέικνται. ὑπὲρ δὲ τῆς ἀκτῆς ταύτης ὁρος ἐστίν, ὃ καλεῖται Κορυδαλλός, καὶ δῆμος οἱ Κορυδαλλεῖς· εἰθ' ὁ Φώρων λιμὴν καὶ ἡ Ψυττάλια, νησίον ἐρημον πετρῶδες, ὃ τινες εἶπον λιμένα τοῦ Πειραιῶς· πλησίον δὲ καὶ ἡ Ἀταλάντη, ὁμώνυμος τῇ περὶ Εὐβοίαν καὶ Λοκρούς, καὶ ἄλλο νησίον, ὁμοίον τῇ Ψυττάλιᾳ καὶ τοῦτο· εἰθ' ὁ Πειραιεύς, καὶ αὐτὸς ἐν τοῖς ἡμοῖς ταπτόμενος, καὶ ἡ Μουνυχία. At the beginning of this passage in the connecting words ἐνταῦθα δὲ καὶ Strabo is still referring to the northern part of the Channel. The twin islands, the Pharmacussae or 'Enchantresses', are to be identified with the modern twins, the Kyrádhēs, and Mt. Corydallus or 'Skylark' with the modern Mt. Skaramangá. The Phoron or 'Smugglers' harbour we may pass over for the moment, in order to consider the islands. If Strabo is, as we should expect, proceeding along the Salamis Channel, he mentions the islands in the order in which they appear, that is to say the islet Psyttalia = Áyios Yeóryios, Atalante = Lipsokoutáli, and the 'other islet like Psyttalia' = Talantónisi, which, being low lying, one does not see until one has rounded Cape Varvári. Then come Peiraeus and Munychia. Once Strabo's order is observed, the identification of Psyttalia = Áyios Yeóryios and Atalante = Lipsokoutáli agrees with the evidence of Pausanias and Stephanus Byzantinus.

There remain, however, some points in Strabo's description which call for comment. He does not mention the island 'Nerá', which lies close inshore to Salamis, either because it was not a land-

<sup>7</sup> Strabo 322 gives the length of his stade. Lolling, p. 7, would amend διστάδιος to δεκαστάδιος, and C. Müller (*FGH V*, p. 1) to δ' στάδιος.

<sup>8</sup> Goodwin, p. 241, reckoned 600 yards of navigable water, allowing for the shoals on each side of the fairway.

<sup>9</sup> See H. Godwin, in *The New Phytologist* 46.1.29 f. (1945), especially p. 65, to whose kindness I owe this information.

<sup>10</sup> Scylax, writing probably c. 338-335 B.C. (*GGM*, p. XLIV), describes Ambracia as up the river and continues ἐστὶ δὲ καὶ ἐπὶ θαλάττης τείχος καὶ λιμὴν κλειστός (ch. 33). I visited the site of Ambracius and found that the footings of the circuit-wall are only a foot or two above the present sea-level and that the enclosed area is all under shallow water. When the sea is rough the walls are sea-washed. It may be assumed that in the fourth century B.C. most of the enclosed area was dry land and the walls were not sea-washed. If follows, then, that since the fourth century B.C. the sea-level has risen by some 4 or 5 feet at least. I noticed evidence of a similar rise in sea-level at Actium and at Treporti on the north shore of the Gulf of Valona, and the sea has risen since antiquity to invade the cave which was excavated by Miss Benton in Ithaca. Mr. R. M. Cook tells me that in the sea-fed lake east of Perachora there are the foundations of an ancient house which are now in sea-water.

<sup>11</sup> The Greek seaman, Mr. Deleyannis, considered that the ship-sheds at the Peiraeus and other foundations indicated a rise of one metre or more in the level of the sea. Mr. E. J. A. Kenny tells me that the slip-way at Sunium seems to extend farther into the sea than is now necessary, but he has no precise

measurements. See note 36 below for the walls at Salamis, and Milchhöfer 7.29 for the presence of walls under the sea on the west side of the Bay of Ambeláki.

<sup>12</sup> That there has been since antiquity no large rise of sea-level (e.g. by some 20 feet), is clear enough from the position of ancient harbours. In the case of the Salamis Channel such a rise would mean that in antiquity the present islands of Nerá, Kyrádhēs, Áyios Yeóryios, and the rock off Péráma would be joined to the adjacent coasts, and Strabo's account would be impossible to explain at all.

<sup>13</sup> The reefs known as Skróphes (off Sclínia) and Arpedhóni (in the northern arm of the Channel) would remain almost the same in size, and neither would be joined to the adjacent coast.

<sup>14</sup> In the short passage, which I have quoted, Strabo uses καὶ, meaning 'also', four times although it is never strictly necessary in logic. This is so in the phrase ὁμοίον τῇ Ψυττάλιᾳ καὶ τοῦτο, where it would be enough to say 'it is like Psyttalia' without adding 'also' to suggest it is rocky and uninhabited. This peculiarity of Strabo's diction is an argument against rather than in favour of emending, as, for instance, Beloch did in supplying after καὶ τοῦτο the words ὁμώνυμον Κίρκος τῇ ἐν ταῖς Κυκλάδαις (Klio 11.432). Milchhöfer 2.46 and Honigsmann (P-W. RE s. Korydallus) believe that Aegaleos was the early name and Corydallus the late name for the same mountain, but it should be noted that Diodorus 4.59.5 probably borrowed it from an early source and Theophrastus used it (Athen. 9.390). In the modern divisions of this area (see sheet 81, 1:100,000 Greek Staff Map, Νόμος Ἀττικῆς Ἐπαρχία Μεγαρίδος) Mt. Skaramangá is in Megaris and Mt. Aegaleos in Attica.



mark as one sailed through the Channel or because in antiquity it was not an island. If we are correct in supposing that the level of the sea was some 5 or 6 feet lower in antiquity, the narrow strip of water between Nerá and Salamis was so shallow that it could easily have been joined to Salamis by a natural deposit or by a causeway. The description of Psyttalia as an uninhabited and rocky islet is appropriate to *Ágios Yeóryios*, which is used today only as a quarantine station and has a rocky scrub-clad surface. Its shores are low, rocky, and surrounded by shallows, so that landing-stages are necessary even for small boats to put inshore and a pier has been built out for the ferry-boat.

In the sentence *ὁ τινες εἶπον λιμένα τοῦ Πειραιῶς* the word *λιμένα* has been emended in all texts to *λήμην* in accordance with Casaubon's suggestion. This emendation is, I think, to be accepted because the explanation of *λήμην* is inherent in the proximity of Psyttalia to Phoron harbour. Demosthenes (932.13-17 and 942.4) explains that Phoron harbour lay outside the bounds of the Peiraeus and that traders rode at anchor there to avoid paying the Peiraeus dues. They would in fact anchor there, just as if one was anchoring at Aegina or Megara (*καὶ ἔστιν ὁμοίον εἰς φαρῶν λιμένα ὁρμίσασθαι ὥσπερ ἂν εἰ τις εἰς Αἰγιναν ἢ εἰς Μέγαρον ὁρμίσαιτο*). In this sense, then, Phoron harbour and Aegina could equally be described as the eyesore of the Peiraeus (Plu. *Pericles* 8, *ὡς λήμην τοῦ Πειραιῶς*). We may assume that the nearby Psyttalia was so-named for the same reason. Modern times provide an exact parallel; ships lie in the Salamis Channel and also in the



FIG. 1.—VIEW FROM XERXES' THRONE ON POINT 57.

Bay of Eleusis to avoid paying the Peiraeus dues. As Phoron harbour is mentioned just before Psyttalia, it should lie north of Psyttalia, probably on the Attic side either at the 'beach of Amphiale' (so named on the Admiralty chart) or at Skaramangá Bay. On the Map, I have put it tentatively at the beach of Amphiale, because the hinterland is best suited for a smuggler's secrecy and there is a spring of water on its south side, the only one in the Channel.<sup>15</sup>

As Strabo defines Psyttalia and the 'other islet like Psyttalia' as uninhabited and rocky, he implies that Atalante was inhabited and therefore larger (the epithet 'rocky' being unimportant, as it is applicable to all the islands). This corresponds to the facts: Lipsokoutáli is a mile or so long, and is inhabited today by Greek naval personnel. The word *πλησίον* should not be pressed to mean more than that Atalante is in the sector between Phoron Limen and the Peiraeus.<sup>16</sup>

Plutarch (*Arist.* 9) mentions *τὴν Ψυττάλειαν ἢ πρὸ τῆς Σαλαμίνος ἐν τῷ πόρῳ κείμενον νῆσος οὐ μεγάλη*. Now 'Salamis' may be used in two senses, as the island or as the town. If it means the island, then *Ágios Yeóryios* is suitable, and, since it lies 'in the strait', more suitable than Lipsokoutáli, which lies strictly outside the strait and off the Peiraeus. If it means the town, the island 'in front of Salamis' can only be *Ágios Yeóryios*; and that this may be the meaning is clear from the context of Pausanias' words *νῆσος δὲ πρὸ Σαλαμίνος ἐστὶ καλουμένη Ψυττάλεια*.

Herodotus (8.76) uses the word Salamis with the same ambiguity when he describes Psyttalia

<sup>15</sup> A further clue to the position of Phoron Limen is afforded by Bekker *Anecdota* 315 (a reference which I owe to the kindness of Mr. C. W. J. Elliot): *φαρῶν λιμένα ὁ φαρῶν λιμὴν ἐστὶν ἐν μεθορίῳ τῆς Ἀττικῆς ἐνθα οἱ λησται καὶ κακοῦργοι ὁρμίζονται*. The boundary between Attica and Salamis probably followed the line of the Channel; in the modern civil divisions the line of demarcation between the Eparchia of Megaris and that of Attica runs west and north of *Ágios Yeóryios* and eastwards through the beach of Amphiale, Salamis island belonging to Megaris. It should be noted that Lipsokoutáli is exposed to

rough seas, is steep-to, and has only one poor landing-place on its north-west side (Milchhöfer 7.29); it is unsuitable for ships to lie to, whether at anchor or not (*ὁρμίζονται*). It cannot, therefore, have been an eyesore of the Peiraeus in the sense which we attach to the word, and it is also likely to have lain within the waters controlled by the Peiraeus authorities in the fourth century B.C.

<sup>16</sup> Strabo often uses *πλησίον* in a very loose and general sense, e.g., of the Rivers Acheron and Thyamis, which are some 30 miles apart (7.7.5, C 324).



as τὴν νησίδα τὴν [Ψυττάλειαν] μεταξύ Σαλαμίνος τε κειμένην καὶ τῆς ἡπείρου.<sup>17</sup> He refers again to the islet (8.95) on the occasion when Aristides collected a force of hoplites who were 'by descent' Athenians (γένος ἔοντες Ἀθηναῖοι) and took them from their station 'along the shore of the Salaminian territory' (οἱ παρετετάχατο παρὰ τὴν ἀκτὴν τῆς Σαλαμίνιας χώρας) across to the islet of Psyttalia. These hoplites were evidently Athenian cleruchs,<sup>18</sup> and the territory was that of the town Salamis, where they held good land, such as exists inland of Kamateró. This passage suggests that the hoplites lined the shore below Salamis town.<sup>19</sup> If so, they could easily see Persian troops moving on Áyios Yeóryios, whereas they could not see men moving on Lipsokoutáli, which, two miles distant, is hidden by the intervening heights of Kamateró and Cape Varvári. It should be noted, too, that the town of Salamis was occupied by the Greeks on the night before the battle (D.S. 11.17.4, ἀπὸ τῆς Σαλαμίνος προθύμως συγκατέβαινον εἰς τὴν ναυμαχίαν).

Aeschylus describes the island on which Xerxes landed his men in *Persae* 447 f.:

νησὸς τις ἔστι πρόσθε Σαλαμίνος τόπων,  
βραία, δύσσορμος ναυσίν, ἦν ὁ φιλόχορος  
Πάν ἐμβατεύει, ποντίας ἀκτῆς ἐπι.

Here again the meaning of 'Salamis' may be held to be ambiguous. If it means the island, then the description is vague; if it means the town, it is precise and can refer only to Áyios Yeóryios.

Site of Ancient  
Salamis town →



FIG. 2.—VIEW FROM XERXES' THRONE ON POINT 57, TOWARDS ÁYIOS YEÓRYIOS.

The epithet δύσσορμος ναυσίν fits Áyios Yeóryios as well as Lipsokoutáli.<sup>20</sup> Áyios Yeóryios, too, is 'close to the ocean strand' of Salamis, whereas Lipsokoutáli is equally well described as 'off Peiraeus'.<sup>21</sup>

<sup>17</sup> The bracket in the Oxford Text may be unnecessary, since Steph. Byz. s.v. *Psyttaleia* quotes Herodotus' words, ἐς τὴν νησίδα τὴν Ψυττάλειαν τὴν μεταξύ Σαλαμίνος κειμένην καὶ τῆς ἡπείρου. The same ambiguity is inherent in most of Herodotus' mentions of 'Salamis' (8.74.1; 76.1; 78; 82.2; 86; 89.1; 90.4; 95-7).

<sup>18</sup> The occurrence of the phrase γένος ἔοντες Ἀθηναῖοι is peculiar in the context of the battle; for it is not used in the many other mentions of Athenians. As their descent is stressed, it is probable that they are Athenian cleruchs at Salamis and were normally known as 'Salaminioi'. A parallel case is supplied by the 4,000 cleruchs at Chalcis. They are referred to as 'Athenians' in contrast to the Eretrians (Hdt. 6.100), but generally, it seems, as 'Chalcidians'. Thus before the battle at Artemisium Athens gave twenty of her ships to the 'Chalcidians' who manned them, the normal full complement being 4,000 men (Hdt. 8.1.2); these 'Chalcidians' fought also at Salamis (Hdt. 8.46.2). Now it is most unlikely that so many native Chalcidians were so friendly or loyal to Athens that they would have fought at Salamis, or indeed that Athens would have entrusted twenty of her ships to the native Chalcidians. This consideration and the similarity in number of the cleruchs and of the men required to man twenty ships make it clear that these 'Chalcidians' were the Athenian cleruchs of Chalcis. In short, then, Herodotus normally named cleruchs not by the city of origin but by the city of residence. In the fourth century we have an interesting parallel in Athenodorus, who was described as an 'Imbrian' and also 'by descent a citizen' of Athens (Plu. *Phoc.* 18; *Dem.* 23.12). There is not space here to develop the argument, but it casts some light upon the

terminology of Thucydides in 7.57.2-3. The Chalcidians named on the serpent-column and in Paus. 5.23 may refer, then, to the cleruchs at Chalcis, and they may have provided the 400 hoplites who fought at Plataea beside contingents from Euboea (Hdt. 9.28.5 and 9.31.4). Bury's suggestion in *CR* 10, 416 f., that these hoplites on Salamis were 'by far the greater part' of the Athenian hoplite army, does not account for the peculiar use of the phrase γένος ἔοντες Ἀθηναῖοι and sets an impossibly high figure for the Athenian population. Athens manned 180 triremes with crews which totalled some 36,000 men drawn from οἱ ἐν ἡλικίᾳ πάντες (Hdt. 8.44.1 and Plu. *Them.* 10, excluding slaves); these, says Bury, were 'a minority' of the whole, but he does not proceed to the deduction that Athens must have had an adult male population of 80,000 or more to justify his statement. The unanimous tradition that the Athenians 'embarked on the ships' (Thuc. 1.18.2) should not be called in question.

<sup>19</sup> To suppose Diodorus to say 'they descended together from the (island of) Salamis' is to postulate an unnecessary vagueness.

<sup>20</sup> This phrase (misquoted by Beloch, *Klio* 8.480) is a periphrasis for 'Salamis'.

<sup>21</sup> Aristodemus 1.4, εἰς τὴν πλησίον νησίδα παροικημένην τῇ Σαλαμίνι ὀνομαζομένην Ψυττάλειαν, may refer to Salamis town or Salamis island; so too Steph. Byz., Ψυττάλεια νησὶς περὶ Σαλαμίνα. He gives an ethnic Ψυτταλέως, indicating that at some time the island was inhabited, although this was not so in the days of Herodotus and of Strabo. Pliny, in *NH* 4.62, 'Salamis, ante eam Psyttalia', is giving a list of islands, and thus refers to Salamis the island.



Lastly, Cynosoura 'the dog's tail' is undoubtedly the long thin promontory with knolls upon it like the vertebrae of a dog's thin tail, which is today called Cape Varvári. Coupled with Cynosoura in Herodotus' account (8.76.1) is Ceos, a name appropriate for an island, which may be tentatively identified with Strabo's anonymous island, mentioned after Atalante, that is to say with Talantónisi.<sup>22</sup>

Having made these individual identifications,<sup>23</sup> we may note that one group of names hangs together. The throne of Xerxes was placed below Mt. Aegaleos, opposite Salamis and above the Heracleum (Hdt. 8.90.4; Plu. *Them.* 13); the Heracleum was on the Attic coast at the narrowest part of the Salamis Channel (Ctesias fr. 26; Aristodemus I.2; Plu. *Them.* 13); men moving on Psyttalia were visible both from the throne of Xerxes and from the shore of the territory of Salamis town (A. *Persae* 465-7; Plu. *Arist.* 9); and Psyttalia lay in front of Salamis town (Paus. I.36.2). This group is of the utmost importance for fixing the scene of the battle. 'The greatest press of ships and the severest fighting took place in the vicinity of Psyttalia, and that was why a trophy was set up on Psyttalia' (Plu. *Arist.* 9). Xerxes selected the site for his throne and placed his men on Psyttalia, because he intended the battle to be fought in the strait (Hdt. 8.76.2, ἐν γὰρ δὴ πόρῳ [τῆς] ναυμαχίης τῆς μελλούσης ἐσσεσθαι ἔκειτο ἡ νῆσος), and in fact he did see the fighting at sea in detail and the slaughter of his men on Psyttalia, as Hdt. 8.86-8 and A. *Persae* 465-7 indicate:

Ξέρξης δ' ἀνῶμωξεν κακῶν ὁρῶν βάθος  
ἔδραν γὰρ εἶχε παντὸς εὐαγῆ στρατοῦ  
ὑψηλὸν ὄχθον ἄγχι πελαγίας ἁλός.

Nor is a high hill near the sea, above the Heracleum and below Mt. Aegaleos opposite Kamateró, hard to find. The photographs are taken from the point 57 (Map 2), the only hill near the sea from which one can see both parts of the Salamis Channel.

All these identifications spring from a direct and simple interpretation of the texts. If we are to make the topographical data the basis of our reconstruction of the Battle of Salamis, we should accept them as they stand without prevarication.

Most discussions of the topography of the Salamis Channel have started from a preconceived plan of the battle. The temptation then is to force the topographical data to fit the plan or to discard or emend such data as do not fit. Beloch, who argued that Psyttalia was Áyios Yeóryios, vitiated his argument by applying a preconceived picture of the Persian fleet facing southwards, by dismissing large portions of the ancient evidence as 'unhistorical' and by emending the text of Strabo (see *Klio* 8.477 f.; 11.432). Most scholars have postulated a battle at or outside the eastern exit of the Salamis Channel. Accordingly, unless they reject the evidence of Plutarch *Arist.* 9, they have to identify Psyttalia with Lipsokoutáli. This identification means that Strabo described the Channel in a most illogical order, namely Kyrádhēs (or, with Beloch, Nerá), Lipsokoutáli, Talantónisi, Áyios Yeóryios (or Nerá), Peiraeus, Munychia. It means that Pausanias at 9.36.2 switched from Salamis town to Salamis island without warning, and that Stephanus Byzantinus gave an ethnic to the sea-washed rock of Talantónisi. It makes nonsense of Diodorus' statement (11.18.2) that in the battle the Greeks held the strait between Heracleum and Salamis. If Xerxes stays on his throne above the Heracleum, he cannot possibly see the detail of the naval battle or his men on Psyttalia-Lipsokoutáli.<sup>24</sup> If his throne is moved elsewhere, then Heracleum and the narrowest part of the strait, together in some cases with Mt. Aegaleos, have to be moved as well unless one rejects the evidence which we have just cited. In this game of tiddlywinks the ancient evidence is rendered almost meaningless; but the moral to be drawn is not that the ancients wrote nonsense, but that the preconceived plans of the battle have little claim to be regarded as historical.

<sup>22</sup> The view advocated by Munro, *CAH* 4, 305, Grégoire, pp. 519f., and Myres, p. 274, that Cape Cynosoura is to be located in the bay of Marathon and Ceos is to be identified with the island off Sunium, is entirely inconsistent with the narrative of Herodotus; it would have been a long and purposeless voyage of up to 70 miles for a part of the Persian fleet to undertake during the hours of darkness until midnight (Hdt. 8.76.1), and it would not have returned in time for the action.

<sup>23</sup> In view of the ancient evidence I do not attach any importance to the occurrence of modern place-names which are similar to ancient place-names. The survival of ancient place-names except in the case of large islands is rare, and their survival at the original site is rarer still; Salamis as the name of a town, Marathon and Corinth are obvious examples of a place-name surviving but migrating to another site. Kleptikolimani, which E. Curtius, *Altertum u. Gegenwart* 2.96, says was a current name for Keratsini Bay, has the same meaning as Phoron Limen, but both arise from smuggling, and a smuggler's choice varies with political and social conditions. As Keratsini Bay is not far from the Peiraeus entrance, it is most unlikely that it was outside the control of the Peiraeus customs authorities

in the days of Demosthenes. The name Talantónisi may represent the ancient Atalante, but in its modern and meaningless form it may have been displaced from its original site by the modern and meaningful name Lipsokoutáli, 'the defective soup-ladle'. We may be warned by the modern name Selinia; on the face of it Σελήνια may be a survival of the ancient name Σελήνια (A. *Pers.* 303), but its present location is one of the few which do not fit Aeschylus' description. Its beach is sandy and not στέγος, and the corpse of Artembares could not have been washed there because the fighting was nowhere near modern Selinia and the wind was westerly when the drift began (Hdt. 8.96.2). See note 85 below.

<sup>24</sup> J. Keil, for instance, p. 334 n. 1, attributes Xerxes' length of sight to 'poetic exaggeration'; but this is not the type of poetic exaggeration to which Attic Tragedy is prone, and Hdt. 8.86 fin., 88.2, 89.2, 90.3-4, which Keil does not mention, cannot be explained away as poetic exaggeration. If Xerxes' throne is moved to within human sight of the fighting on Psyttalia-Lipsokoutáli, it has to be taken to the hills beside the Peiraeus, which dissociates it from Mt. Aegaleos.



## II. THE EVIDENCE FOR THE BATTLE

When the fighting was over, no official and authoritative history of the action was compiled. Eyewitnesses told their own stories and some wrote their own accounts, each recording the events which he had seen; Aeschylus was not the only one to do so, for the Persian war was treated by authors earlier than Thucydides (Thuc. 1.97.2; D.H. *de Thuc.* 5, naming eight historians earlier than Thucydides). The details which they recorded were probably different, because they saw different parts of the action, but that does not mean that they were not correct. These eyewitness accounts were presumably the original source of later historians and the indirect source of our information, and they have a strong claim to be regarded as trustworthy.

It is probable that no one account was comprehensive. The *Persae* of Aeschylus, who fought in the battle and presented his play eight years after the event, is a good example. His account, being confined to the compass of a messenger's speech, is very far from comprehensive, so that it is absurd to question a point in another account simply because it does not figure in the *Persae*. Even within its own limits the tempo of the action is accelerated by Aeschylus for dramatic purposes. The prose-writers (Hdt. 8.84; D.S. 11.18.3-6; Plu. *Them.* 14) describe a delay before the ships engaged, but Aeschylus presses on to the next vivid incident in the action (406-9):

καὶ μὴν παρ' ἡμῶν Περσίδος γλώσσης ῥόθος  
ὑπηγνίσσε, κούκέτ' ἦν μέλλειν ἄκμή.  
εὐθύς δὲ ναῦς ἐν νηὶ χαλκίῃ στόλον  
ἔπαισεν.

Again, after the battle, when the prose-writers relate the slowness of Xerxes' movements, Aeschylus presses on to the sequel (480-3):

ναῶν δὲ ταγοὶ τῶν λελειμμένων σύδην  
κατ' οὖρον οὐκ εὐκοσμον αἶρονται φυγῇ·  
στρατὸς δ' ὁ λοιπὸς ἐν τε Βοιωτῶν χθονὶ  
διώλλυθ'.

Here I see no conflict of fact between Aeschylus and the prose-writers but only an example of Aeschylus' dramatic skill.

Herodotus, of course, wrote a much fuller account, based upon eyewitness accounts and written or recited for an audience which still included many who had participated in the battle. These conditions give us a strong guarantee of accuracy. Yet his account is not comprehensive; for his space was limited and he was concerned with the decisive part of the action. For example, he mentions but does not describe the position of the Persian fleet on the day before the battle (8.70 and 78); he refers to the Corinthians' account of their part in the fighting without retailing it (8.94.4); and he makes no mention of an attempt by Xerxes to build a mole before the battle, although he mentions an attempt later to 'make' or perhaps to 'complete a mole to Salamis' (ἐς τὴν Σαλαμῖνα χῶμα ἐπειράτο διαχοῦν 8.97.1).<sup>25</sup> It does not follow that these matters were not fully known to Herodotus, and even less that they were not known to or not recorded by others.

The details, which are supplied by Ctesias, Diodorus, Plutarch, Aristodemus, and others, were probably derived ultimately from contemporary or nearly contemporary accounts. For it is most unlikely that new details or indeed new plans of the battle, which was more famous in Greek history than the defeat of the Spanish Armada in English history, were 'invented' by subsequent historians and in particular by as reputable a historian as Ephorus, who was probably the immediate source of Diodorus' narrative. In this paper these details are accepted as correct, and the various accounts are regarded as complementary and are not set against one another in competition.<sup>26</sup>

In considering the details of the action, as in handling the topographical data, it is tempting to begin with a preconceived picture of the battle. The danger of doing so is apparent from the summary of views which is given in Kromayer-Veith, *Antike Schlachtfelder* 4.65 f. The modern reconstructions of the battle are there divided into two groups, those of the 'Frontalschlacht' and those of the 'Einkreisungsschlacht'. This division is then projected back into the ancient accounts: 'der moderne Gegensatz zwischen den beiden in Frage stehenden Auffassungen von der Schlacht geht bereits auf unsere Quellen aus der Altertum zurück' (p. 78). Herodotus and Diodorus are put into the ring as the ancient protagonists of this modern controversy. Their accounts are treated as if each was comprehensive, but even so, adjustments have to be made. This approach towards the ancient evidence does indeed enable one to pick and choose at one's discretion. Chunks of

<sup>25</sup> The verb διαχοῦν is translated in *L-S-J* 'to complete' a mole, but Strabo (9.1.13, C 395), does not use it in this sense.

<sup>26</sup> By assuming that the accounts of Aeschylus and Herodotus are both comprehensive, most scholars detect inconsistencies between them. They also dismiss the evidence of Diodorus as 'built up by the reflection, inference, rationalism and con-

jecture' of Ephorus (e.g., Munro, *JHS* 22, 239); but this is to overlook the possibility, and I think the probability, that Ephorus drew on sources independent in some respects of Aeschylus and Herodotus. The same may be said of Plutarch. A less severe attitude towards Aeschylus, Herodotus, Diodorus, and Plutarch was taken by Goodwin 245 f.



varying size are jettisoned. Beloch,<sup>27</sup> for example, discarded the entire story of Sicinnus' message and Themistocles' trick; Tarn<sup>28</sup> proposed to reverse the timing of the landing on Psyttalia and the opening of the action; and Grégoire<sup>29</sup> denied that the Persian fleet ever concentrated at all at Phalerum. In detail, too, the evidence can be emended as corrupt<sup>30</sup> or discarded as untrustworthy<sup>31</sup> or omitted from consideration.<sup>32</sup> By such methods the ancient evidence can be forced into some congruity with a preconceived plan of the battle. But again the doubt arises whether the plan has the prior claim to be regarded as historical.

The same is true of the numbers of ships engaged in the battle. It is easier, but not therefore more true to historical fact, to whittle down the numbers before reconstructing the battle.<sup>33</sup> In my opinion the statement in Aeschylus *Persae* 334-43 commands respect. A participant himself in the battle, addressing fellow-participants eight years after the event, Aeschylus put on the lips of Atossa the vital question about the Persian defeat at Salamis: 'How great was the number of the Greek ships that they dared with their rams to engage the Persian host?' The answer is 310 Greek ships facing 1,207 Persian ships in the battle.<sup>34</sup> Because Aeschylus ascribed the victory to the divine will, he was honest and impartial in his record of the facts. Moreover, he gave precise, not approximate numbers, and it is beyond dispute that he believed them to be correct. Nor is there any reason why he should not have known the correct numbers. Before the battle the Greek and Persian squadron-commanders certainly knew the numbers of their own fleets; otherwise planning and manoeuvring would have been impossible. After the battle there was no reason for keeping the numbers secret, if indeed they were ever secret. The Greeks had every opportunity to learn



FIG. 3.—VIEW FROM XERXES' THRONE ON POINT 57, TOWARDS THE MODERN NAVAL BASE.

the number of the Persian fleet before the event from spies, prisoners, and deserters and after the event from prisoners and the many Ionians who entered the Delian Confederacy. All in all, Aeschylus' figures are dependable and, being in verse, safe from emendation.

Aeschylus gives the numbers engaged in the daring Greek attack on the Persian fleet. According to other authorities, both sides had detached a contingent *before* the engagement began, the Greeks a contingent of unspecified size under Adeimantus (Hdt. 8.94.1) and the Persians one of 200 ships (Plu. *Them.* 12). If we compare Herodotus' figure of 380 ships for the Greek fleet *before* the engagement began (8.82.2) with Aeschylus' figure of 310 ships *when* the engagement began, it is clear that the contingent under Adeimantus numbered 70 ships. If we combine Aeschylus' figure with that of Plutarch, we have a total of 1,407 ships for the Persian fleet before the engagement began. A figure of the same order is suggested by Herodotus; for, after allowing for losses by storm and off Artemisium, he gave it as his considered opinion that the Persian fleet before the battle was no less numerous than it had been at Sepias (8.66), that is no less numerous than a total of 1,327 ships approximately (7.184.1 and 185.1). If there was a discrepancy between Aeschylus and Herodotus we should give the priority to Aeschylus; but there is no discrepancy, and this is the more reassuring because Aeschylus is not the source of Herodotus, when Herodotus mentions 1,327 ships at Sepias and 380 Greek ships at dawn before the engagement began at Salamis.

<sup>27</sup> *Klio* 8, 484 f.

<sup>28</sup> *JHS* 28, 226; the only support for his view is in Aristodemus 1.4, a passage clearly due to an error of understanding and not mentioned by Tarn.

<sup>29</sup> *Études classiques* 4, 519 f.; see the answer to Grégoire by Legrand, pp. 55 f.

<sup>30</sup> Löschcke, *Jahrb. f. Philol.* 115, 25; Beloch, *Klio* 13, 130; Wilhelm, *SB Akad. Wien* 211.1.28; Jacoby, *Philologus* 86, 369; J. Keil, p. 349.

<sup>31</sup> E.g., by J. Keil, p. 335.

<sup>32</sup> J. Keil, for instance, does not consider the evidence for the Greek position which is contained in *Persae* 386-98.

<sup>33</sup> For instance, Tarn, p. 226, held that the Greeks outnumbered the Persians, which is a bold reversal of the evidence. A healthy reaction against such methods has begun; for in the most recent discussion by Labarbe, pp. 421 f., Herodotus' figures for the Greek fleet are accepted.

<sup>34</sup> The interpretation which I give of A. *Pers.* 337-44 is in accordance with the normal meaning of *χωρίς* and of *πέν* and *ἑξ*.



The ancient evidence has been regarded with suspicion, mainly because the figure 1,207 appears twice in a different context in Aeschylus and in Herodotus. It is given by Aeschylus as the strength of the Persian fleet in the actual engagement at Salamis, and by Herodotus as the strength of the Persian fleet which sailed from Asia and was reviewed at Doriscus (7.184.1 and 7.89.1). Three explanations are possible. Either the Persian fleet was in fact 1,207 ships strong at Doriscus and 1,207 + 200 ships strong before the engagement at Salamis. This might be so if Xerxes obtained large naval reinforcements from his dependencies throughout the Eastern Mediterranean. In that case the double occurrence of the number 1,207 is due to the arm of coincidence, which is sometimes long. Or Herodotus was mistaken in giving the figure of 1,207 ships for the review at Doriscus. This explanation is more probable if we suppose Herodotus to have found this figure in the source which he used for the review as a whole; for the fantastic numbers given for the Persian Army suggest that the source was far from dependable. Or Aeschylus was mistaken in applying to the opening phase of the engagement at Salamis a number which really applied to the review at Doriscus. Such a mistake is very improbable. Aeschylus lived through the eventful months of war, he knew of the Persian losses in storms and in action, and he cannot have believed that the number of the Persian ships at Doriscus (if he ever knew it) was *ipso facto* the number of the Persian ships in the engagement at Salamis. Both he and his audience were much too interested to have entertained such a belief. If, therefore, a critical historian wishes to reject one of the two occurrences of the figure 1,207, it is more in accord with historical principles to reject that given by Herodotus than that given by Aeschylus.



FIG. 4.—VIEW FROM HILL ON SALAMIS WITH *Ágios Yeóryios* IN CENTRE OF CHANNEL.

In what follows I accept these traditional numbers for the two fleets.<sup>34a</sup> At the same time it may be noted that the nature of the battle itself is not much changed, if one prefers to whittle the number of Persian ships down to 800 or so.

### III. THE MANOEUVRES OF THE DAY AND THE NIGHT BEFORE THE BATTLE

The Persian armada with the sole exception of the Parian squadron concentrated at Phalerum Bay (Hdt. 8.66–7), where the sandy shore was suitable for beaching and good anchorages were close at hand, at Tourkolímano and the Peiraeus, and there was space for launching and deploying in open waters. The Greek fleet of nearly 380 triremes lay somewhere inside the Salamis Channel; for the Greek commanders intended to hold the narrow strait, because in the words of Herodotus (8.60 b) 'to fight in the narrows is to our advantage, but to fight in open waters is to their advantage'.

<sup>34a</sup> On the disputed passage in Thuc. 1.74.1, *παρασχομένη* . . . ναὺς μὲν γὰρ ἐς τὰς τετρακοσίας (or with one codex *τριακοσίας*) ὀλίγω θάλασσοις τῶν δύο μοιρῶν, several views are possible. Gomme, *op. cit.* 1.234, suggests reading *πρὸς τὰς τριακοσίας* or *τετρακοσίας*, which would dispense with the rather meaningless γὰρ and is closely paralleled by the phrase in Hdt. 8.44: 'Ἀθηναῖοι μὲν πρὸς πάντας τοὺς ἄλλους παρεχόμενοι νῆας ὀγδῶκοντα καὶ ἑκατὸν μοῖναι. This would mean that the Athenians, providing 200 ships at Salamis, claimed to have provided a little less than two-thirds of 300, i.e., less than 200, which is correct if they did in fact provide 180 (Hdt. 8.44); on the other hand, if they claimed to have provided rather less than two-thirds of 400 ships, this being a round figure for 380, the claim is an extravagant one, even if the twenty ships of the Chalcidian cleruchs are added. Labarbe, p. 419, keeps the text and reads *τριακοσίας*,

interpreting the passage to mean that Athens supplied up to 300 ships (in fact, some 270 on his calculation) and the total Greek fleet was some 450 ships; he then refers these figures to the beginning of the war. The objection to his view is less in the figures than in the context; for Thucydides appears to be speaking of the decisive Battle of Salamis. A third view is to keep the text, but assume that the Athenian claim is made with reference not to triremes but to warships of all kinds; on this interpretation the Greeks had a total of more than 600 ships and Athens supplied up to 400 ships, and the former figure is in accordance with Ctesias' statement that the Greeks had 700 ships at Salamis. But, whatever is done with this passage in Thucydides, the evidence of Aeschylus and Herodotus is more authoritative.



In choosing their station within the Salamis Channel the Greek commanders had to bear in mind the facilities for beaching, because the triremes were hauled on land for the night. In addition to the 380 triremes there were many auxiliary craft (Plu. *Arist.* 9 ὑπηρετικά), both those which operated with the fleet and others which had been brought over from Attica in the evacuation; whether we accept the figure of Ctesias fr. 26—700 ships—or not,<sup>35</sup> the total number of vessels within the Channel must have been of that order. As speed in launching was important, we should allow some 10 yards of beach for each trireme<sup>36</sup> and a single line or at most two lines of triremes on the beach; for a total of 380 triremes we therefore need just over 2 miles of beach if they were in single line, or about 1 mile of beach if they were in two lines. If we begin from the eastern end of the Channel, we may exclude the north shore of Cape Varvári which is of sharp limestone and impossible for beaching.<sup>37</sup> The shores of the Bay of Ambeláki are suitable, but, if the sea-level was lower in antiquity by some 5 feet,<sup>38</sup> they provided room for a part only of the fleet; moreover, the bay itself would afford little room for the mustering of a fleet launched from its surrounding shores. The Bay of Paloukia, including the north-west part of Kamateró, has some 2 miles of suitable beaches which face the open Channel; there the whole battle fleet could be beached or launched rapidly. Farther up the Channel the shores of the Bay of Arápis are now built up with wharves for the modern Greek Navy; in antiquity these shores may have been suitable for beaching, but they are less extensive and less close to ancient Salamis town than the beaches of Paloukia. Proximity was important, because the Greek troops bivouacked at night in Salamis town, that is on Kamateró promontory (D.S. 11.17.4). Another problem was water for the naval personnel, who numbered considerably more than 75,000. The east side of the island has no springs, and the best supply of well-water today is in modern Salamis town on the west side of the waist of the island. To this point of supply Paloukia is closest. I conclude, therefore, that the Greek battle-fleet was based on Paloukia Bay.

This station accorded with the strategy of the Greek commanders. For they intended to fight in the narrows, which I take to be at the bend of the Channel between Kamateró, Áyios Yeóryios, and Péráma. These narrows are presumably the πόρος (Hdt. 8.76.2), in contrast to the πορθμός (Hdt. 8.76.1; 8.91) or vestibule stretching eastwards of the narrows as far as Munychia.<sup>39a</sup> If the Greek commanders could tempt the Persians to attack them in the narrows, they would reap three advantages: their own front would be narrow, their hoplites on the shore of Kamateró would cover their right flank, and the Persian fleet would have to enter the comparatively narrow vestibule between Cape Cynosoura and the Attic coast to the north. If, on the other hand, they sailed out to hold the waters between the tip of Cape Cynosoura, Lipsokoutáli, and the Attic coast to the east, they would obtain no support from their hoplites, and the Persian fleet would remain in the open waters south of Lipsokoutáli. Thus the Greek station at Paloukia Bay was admirably chosen.

When Xerxes had captured Athens, his first aim was to defeat the Greek fleet. But the Greek position at Salamis seemed to him too strong for a frontal attack by his own fleet. He therefore decided to prise the Greek fleet out of its position. He hoped to bring this about by throwing a mole across the narrows from the Heracleum near Péráma. Once the mole was nearing completion, the Greek fleet would be compelled to move either eastwards into the vestibule, where the Persian fleet would have the advantage of wider waters, or into the northern part of the Channel. If the Greek fleet stayed in the northern part of the Channel, the next stage would be to complete the mole, to land infantry on the island, and to capture the base of the Greek fleet. Once this was achieved, the Greek fleet would either be starved into surrender or have to fight in the wide waters of the Bay of Eleusis.

This first plan of Xerxes is clearly stated in Ctesias fr. 26, Strabo 395, and Aristodemus 1.2. The fullest account is that of Ctesias: ὁ δὲ Ζέρξης αὐτόθεν ('from Athens') ἐλθὼν ἐπὶ στενότητον τῆς Ἀττικῆς—Ἡράκλειον καλεῖται—ἐχώννυε χῶμα ἐπὶ Σαλαμίνα, περὶ ἣν αὐτὴν διαβῆναι διανοούμενος· βουλῇ δὲ Θεμιστοκλέους Ἀθηναίου καὶ Ἀριστείδου τοξόται μὲν ἀπὸ Κρήτης προσκαλοῦνται καὶ παραγίνονται· εἶτα ναυμαχία Περσῶν καὶ Ἑλλήνων γίνεται. The line of the proposed mole was from the Heracleum to Salamis town, i.e., via Psytalia, and the method of its construction is known from the description of the renewal of the attempt in Herodotus 8.97.1. Phoenician merchantmen were chained together to form at the same time a breakwater, a wall of defence, and a raft for the workmen. This technique was compared by Aristodemus to a more difficult feat of engineer-

<sup>35</sup> Labarbe p. 425 n. 5, holds that the word ναὺς in the epitome of Ctesias must mean a trireme, but this technical meaning may not be necessary in an abbreviated version.

<sup>36</sup> Myres, p. 281, allows 7 yards, which seems to be too tight, if one envisages the launching of a heavy ship by manual methods. For the hull amidships was 5 yards wide (see p. 50 below for the dimensions of the trireme).

<sup>37</sup> The jagged rocks of Cynosoura promontory, which are apt to cut a bather's feet, begin at the knob of rock on the south side of the mouth of Ambeláki Bay; they render the station of the Greek fleet proposed by Munro (CAH 4, map 9) completely impossible.

<sup>38</sup> It is unlikely that there has been any marked silting up of the Bay of Ambeláki since antiquity, because the area draining

into it is so small. Moreover, the remains of Hellenic walls (shown in Lolling, p. 9 and map facing p. 10) are now close to the sea and almost at sea-level. If the walls were built to enclose a habitable area, then the coast-line was more distant in antiquity than today and the sea-level was correspondingly lower (see n. 10 and 11 above for indications of a change in sea-level).

<sup>39a</sup> This is consistent with Herodotus' usage of πόρος for the narrows of a strait, e.g., 7.176.1 and 183.3 between Sciathos and the mainland and 7.34 from Abydos to the European coast. It is interesting that Aeschines 3.158, in speaking of the ferry-men who plied to Salamis (τοὺς εἰς Σαλαμίνα πορθμύοντας), described the water they crossed as ὁ πόρος.



ing, the bridging of the Hellespont (Hdt. 7.36). At the Heracleum Xerxes posted a part of his army (Aristodemus 1.2: μέρος τι ἔχων ἦκεν κατὰ τὸ Ἡράκλειον). This force no doubt covered the passage of the merchantmen along the Attic shore to the Heracleum by night and perhaps by day, despite the proximity of the Greek fleet.

When the bridging was arrested by the Cretan archers, who presumably fired from the decks of the Greek ships, Xerxes rejected Artemisia's advice to delay and decided to offer battle at sea. But he did not bring his fleet into the Salamis Channel. His squadrons put out from Phalerum 'towards Salamis' and deployed in the open waters, presumably extending from off Peiraeus and Munychia to south of Ceos. There they were seen by the Greek observers. Their dispositions were not regarded by the Greek commanders as constituting an immediate threat to the Greek fleet, which lay up the Channel at the narrows (Hdt. 8.70 and 78). Towards evening the Persian fleet returned to its base, and the crews landed to prepare the evening meal (A. Pers. 374-9; Hdt. 8.70).<sup>39</sup>

The movement of the Persian fleet had alarmed the Greek captains, who feared that Xerxes would force an engagement and that they, if defeated, would be cut off and besieged on the island (Hdt. 8.70). Towards nightfall the main body of the Persian army began to move towards the Peloponnese (Hdt. 8.71); this movement probably had two purposes, to occupy the Attic coast facing the Bay of Eleusis and to increase the apprehension of the Peloponnesians in the Greek fleet, who were likely to be informed by refugees from Eleusis. In any case the Greek captains were thoroughly alarmed. Themistocles then sent his messenger Sicinnus to the Persian fleet. His report, that the Greeks were demoralised, were contemplating flight, and would either make no resistance or fight among themselves, was accepted as true by the Persian captains and by Xerxes himself (Hdt. 8.74-76; A. Pers. 355-362; cf. Thuc. 1.137.4).

Whatever plans Xerxes had had in mind for the next day, he now issued new orders. During the hours of darkness three movements were to be executed. A detachment of picked Persian troops, numbering some 400 according to Pausanias 1.36.2, were to occupy the desert island of Psyttalia. We may imagine them setting out from the mole near Péræma in small boats, which, moving against the background of the high Attic coast, would have been hard to discern from the Salaminian shore. The Persians crossed in silence, and took up positions on Psyttalia undetected (Hdt. 8.76). A squadron of the Persian fleet was to patrol the western bay of Salamis island, that is to say the bay off the modern town of Salamis, and to close the narrows between Salamis and Megara (A. Pers. 368: ἄλλας δὲ κύκλῳ νῆσον Αἶαντος πέριξ; Hdt. 8.79.4: περιεχόμεθα γὰρ ὑπὸ τῶν πολεμίων κύκλῳ); this squadron was to sail at once, while the other two movements were still in preparation (Plu. Them. 12). The main Persian fleet, numbering 1,207 ships, was to guard the (eastern) exits and the sea-routes, in order to close off all means of escape for the Greek fleet (A. Pers. 366-7 and 369 f.; Hdt. 8.76.2).<sup>40</sup>

As the main fleet drew out towards the island, it moved in silence to escape observation (Hdt. 8.76.3), and in fact the Greek captains remained unaware that they were being encircled, supposing the enemy fleet to be in the position which they had seen it holding during the day (Hdt. 8.78).<sup>41</sup> The Persian fleet presumably moved to a position where it could intercept any Greek ships which sought to steal out under cover of darkness. The best and shortest route for such an escape was the channel between Cynosoura and Ceos, and thence into the open sea between Salamis and Aegina. Accordingly, a part of the Persian fleet patrolled in battle-order 'around Ceos and Cynosoura'

<sup>39</sup> Hdt. 8.70.1 and 76.1 is more compendious than Aeschylus, but he clearly indicates an interval between the approach of nightfall and the operations at midnight, during which the Persian fleet was making its preparations not in the open sea but at its base.

<sup>40</sup> The same manoeuvre is described in more grandiose terms by Plutarch *Arist.* 8: αἱ βαρβαρικάι τριήρεις νύκτωρ ἀναχθεῖσαι καὶ περιβολοῦσαι τὸν τε πόρον ἐν κύκλῳ καὶ τὰς νήσους κατεῖχαν, αὐθεντὸς προειδότες τὴν κύκλωσιν . . . . τὸ γὰρ ἐν κύκλῳ καὶ κατόπιν ἦδη πῆλαγος ἐμπέτησται νεῶν πολεμίων . . . φυγῆς γὰρ ὁδὸς οὐ λείπειται and *Them.* 12 διακοσμίαις δ' ἀναχθέντας ἦδη περιβαλίσθαι τὸν πόρον ἐν κύκλῳ πάντα καὶ διαζῶσαι τὰς νήσους ὅπως ἐφυγῇ μηδεὶς τῶν πολεμίων . . . φράζει τὴν κύκλωσιν.

<sup>41</sup> The inference to be drawn from this statement is that the visibility was such that the Greeks did not see the part of the Persian fleet which was closing the eastern exit of the Salamis Channel. On the other hand, they apparently saw some Persian ships to the south of Ceos, a sight which led them to suppose the Persians to be in the same position as on the preceding afternoon. Otherwise it is difficult to understand how they believed the Persians to be in this position during the night. This raises the question of moonlight on this night. Busolt, *GG* 2<sup>2</sup>, 702, Beloch, *GG* 2<sup>2</sup>, 2, 47 and Myres, p. 270, state that 'the moon rose at midnight or after', but they do not explain why Xerxes waited for moonlight before he put his fleet out in an advanced position, hoping to escape observation. Goodwin, p. 242, refers to the two dates given by Plutarch for the battle, namely '16th Munychion' and 'about 20th Boedromion', and to his statement that the Greeks dedicated

the 16th Munychion to Artemis: ἐν ᾗ τοῖς Ἕλλησι περὶ Σαλαμίνα νεώσων ἐπέσκηψεν ἡ θεὸς πανσέληνος (*de Glor. Ath.* 7 and *Camillus* 19). As Munychion corresponds roughly to April and Boedromion to September, the mention of Munychion in Plutarch is clearly an error, and we may conclude either that the passage is to be rejected in its entirety or that it is an error only of Munychion for Boedromion (the same error occurs in Plu. *Lys.* 15, where he puts the Battle of Salamis and the intervention of Lysander in 404 B.C. on 16th Munychion instead of in Boedromion, for which date see A. Fuks, *The Ancestral Constitution*, p. 70). If the latter alternative is adopted, we are left with two dates in Boedromion, the 16th and 'about the 20th'. Moreover, the 16th of Boedromion is the date given in Polyaeus 3.11.2. The moon was full on the 16th and was waning on the 20th; in either case the moon rose early in the night and set late. As the moon rose in the east it would be riding in the south about midnight. During a moonlit night at sea one benefits by the reflection of the moon's rays off the water to see a fleet between oneself and the moon. In order to deprive the Greeks at Salamis of this advantage, Xerxes waited until midnight to bring his fleet forward; and he moved his force of Persians on to Psyttalia when the moonlight was not to his disadvantage. In this calculation there are several imponderable factors: we do not know whether Plutarch's statement is dependable in its confused form and whether the night before the battle was cloudy, hazy, or clear. If I am correct in believing that a south wind was blowing out at sea, the probability is that the sky was cloudy or hazy during the night.



(Hdt. 8.76.1). But, if Xerxes hoped to intercept escaping ships in these waters, he also intended to advance at dawn into the Salamis Channel and attack the Greek fleet. In preparation for this advance the centre and the right of the Persian fleet were out at sea, the right wing swinging into a position west of north<sup>42</sup> and towards Salamis (Hdt. 8.76.1; cf. 83.2 fin.). Thus, when midnight came, the main body of the Persian fleet was already poised for interception and, later, for advance. These movements are clearly described by Herodotus 8.76. τοῦτο δὲ, ἐπειδὴ ἐγίνοντο μέσα νύκτες, ἀνῆγον μὲν τὸ ἀπ' ἐσπέρας κέρας κυκλοῦμενοι πρὸς τὴν Σαλαμίνα, ἀνῆγον δὲ οἱ ἀμφὶ τὴν Κέον τε καὶ τὴν Κυνόσουραν τεταγμένοι . . . . τῶνδε δὲ εἵνεκα ἀνῆγον τὰς νέας, ἵνα δὴ τοῖσι Ἕλλησι μηδὲ φυγεῖν ἐξῆ . . . ἐς δὲ τὴν νησιδα τὴν Ψυττάλειαν καλεομένην ἀπεβίβαζον τῶν Περσέων τῶνδε εἵνεκα, ὥς ἐπεὶ γένηται ναυμαχίη ἐνθαῦτα μάλιστα ἐξοισομένων τῶν τε ἀνδρῶν καὶ τῶν ναυηγίων (ἐν γὰρ δὴ πόρῳ [τῆς] ναυμαχίης τῆς μελλούσης ἐσεσθαι ἔκειτο ἡ νῆσος), ἵνα τοὺς μὲν περιποῖωσι, τοὺς δὲ διαφθεῖρωσι. ἐποίουν δὲ σιγῇ ταῦτα, ὥς μὴ πυνθανοῖατο οἱ ἐναντίοι.<sup>43</sup>

If Sicinnus' information was correct, the Persian strategy was good. Two phases were envisaged. Between midnight and dawn any escaping ships would fall into the widespread net. Shortly after dawn the main Persian fleet would descend in force upon the Greek naval base, which lay some three miles distant from their leading ships. If the surprise was complete, as the Persians expected it to be, they would catch many Greek ships on the beaches and engage any which offered battle in the vicinity of Psytalia, where the Persian force was in possession (ἐν γὰρ δὴ πόρῳ [τῆς] ναυμαχίας τῆς μελλούσης ἐσεσθαι ἔκειτο ἡ νῆσος (Hdt. 8.76.2; cf. A. Pers. 450 f.).

The position of the main Persian fleet at midnight can be fixed approximately and not precisely. The left wing cannot have been close inshore to Cynosoura; for the Greeks may be presumed to have had observers on the peninsula,<sup>44</sup> and the Persians intended to escape observation. The right wing, on the other hand, may have been close inshore to the Attic coast, which Xerxes held. The centre, however, must have been held back away from Cynosoura, in order to escape observation. The front line or the two front lines may have lain west of and close inshore to Atalante, but the supporting files lay east of Atalante (see Map 1). As the first phase was to lead immediately to the second phase, the disposition of the squadrons during the night was maintained for the attack at dawn; the Phoenicians held the right wing, the Ionians the left wing, and the other nationalities the centre (Hdt. 8.85; D.S. 11.17.3). During the night, when the armada was patrolling under oar, the ships were in open order, so that their formation reached in depth to Munychia (Hdt. 8.76.1 fin.). At dawn they were, no doubt, to move into closer order and advance to the attack.

Aeschylus *Persae* 366 describes the Persian formation at night as a 'close array in three columns' (τόξαι νεῶν στίφος μὲν ἐν στοίχοις τρισίν). The word στοίχος has been taken to mean a single line ahead. But a formation of three ships abreast and 400 ships deep is an obvious absurdity; for a front of three ships could not block the exit at all, and, if an action developed, the rearward ships would be more than 10 miles away from the leading ships. 'The three columns' probably refer to the three squadrons, Phoenician, Ionian, and mixed, each being under one admiral (cf. D.S. 11.17.2-3: διατεταγμένοι κατὰ ἔθνος ἐξῆς). The meaning of στίφος is fortunately not in doubt. The ships formed 'a close array', which is quite incompatible with a formation of only three ships abreast, and, when they advanced into the Channel, they moved like 'a stream', filling the Channel from shore to shore (τὰ πρῶτα μὲν νυν ῥεῦμα Περσικοῦ στρατοῦ ἀντεῖχεν, A. Pers. 412).

At this point in his narrative Herodotus introduces the oracle of Bacis (8.77), which, as he remarks on coming to the end of the battle, was entirely fulfilled (8.96.2).<sup>45</sup> I take it, therefore, that

<sup>42</sup> Herodotus names as 'west' any direction which is west of north (cf. 7.96.2 and 7.176.3).

<sup>43</sup> I translate the full passage as follows: 76.1—'On the one hand, they landed many of the Persians on the islet Psytalia which lies between Salamis and the mainland. On the other hand, when midnight came, they were moving out at sea, curving their western wing on the one hand towards Salamis and being already in formation on the other hand off Ceos and Cynosoura, and they occupied the entire strait with their ships as far as Munychia.' 76.2—'They were moving the fleet out at sea with the intention of preventing the Greeks from escaping and of ensuring that they should be cut off at Salamis and pay the penalty for their actions at Artemisium. They were landing some of the Persians on the islet Psytalia with the intention that, when a battle developed and men and wreckage were cast up mainly in that area (for the island lay in the narrows where the battle was likely to take place), the Persians would aid their friends and destroy their enemies.' 76.3—'They were executing these movements in silence, so that they should not be observed by their opponents.' While the aorist tense of ἀνάγειν or ἀναγεσθαι with or without τὰς νέας means 'to put out to sea' from the shore (e.g., 3.41.2 ἀναγαγεῖν, 7.100.3 ἀναγαγόντες, 3.138.1 ἀναχθέντες), the imperfect is used of ships moving out at sea (e.g., 8.70 ἀνῆγον τὰς νέας ἐπὶ Σαλαμίνα, and in the participles 6.12.1 ἀνάγων ἐκαστοτε ἐπὶ κέρας τὰς νέας, 8.83.2 ἀναγόμενοι). The use of κέρας shows that Herodotus is speaking of a continuous formation and not of a detached

squadron, and the μὲν and δὲ refer to the two wings or extremities of this continuous formation, which extended in depth towards Munychia. The imperfect tenses in 8.76.1 mark the continuous movement of the fleet (cf. A. Pers. 382, καὶ πένωνται δὴ διαπλοῶν καθίστασαν). The purpose of the movements is clearly stated in 8.76.2, and the need for silence was due to the proximity of the enemy. I find no difficulty in the Greek of this passage, which was a stumbling block to Goodwin (pp. 251 and 261-2) and others in their plan of the whole engagement.

<sup>44</sup> I noticed the remains of a tower at the spot marked X on map 2, which does not figure on Milchhöfer's map, *op. cit.* Bl. XXI. The masonry is probably of late-fifth- or early-fourth-century date, and the occasion for its construction may have been the Corinthian War, when a Spartan fleet lay at Aegina; it would serve as an observation post and as a blockhouse against a landing of troops designed to raid Salamis town. Greek observers on the night before the battle may have been posted on this part of the peninsula rather than on the tip, which is less high.

<sup>45</sup> The question, whether the oracle was a *catinicum post eventum* or not, cannot be resolved; but it should be noted that the collection of Musaeus' oracles from which it came (Hdt. 8.96.2) was in general of earlier origin, that the Greeks themselves were aware of one case of tampering with oracles (Hdt. 7.6.3), and that Herodotus himself was convinced that this oracle was *ante eventum*. But the question is not relevant to the bearing of the oracle on the position of the Persian fleet.



the oracle is placed here proleptically, in order to inform us beforehand of the impending issue, which is made manifest in the action of the next day. In some respects the meaning of the oracle is obscure, as we should expect. It may be translated literally as follows:

'But, whenever, after sacking brilliant Athens in their mad ambition, they bridge <sup>46</sup> the sacred shore of gold-accounted Artemis and seawashed Cynosoura with their ships, divine Justice will extinguish bold Presumption, son of Insolence, in his dreadful lust purposing to overthrow all things; for bronze will clash with bronze, and Ares will empurple the sea with blood. Then far-seeing Zeus and revered Victory bring the day of Greece's freedom.'

At the town of Salamis there was a sanctuary of Artemis (Paus. 1.36.1). As Herodotus put the battle off Salamis and believed the oracle was confirmed by the event, I take it that in the course of the battle the Persians 'bridged (the waters to) the shore' of Salamis town and Cape Cynosoura. The point of departure is not expressed in the oracle, but Herodotus naturally took it to be Attica.<sup>47</sup> Thus on the next day we expect the Persian fleet to fill the Channel from shore to shore in the vicinity of Salamis town and Cape Cynosoura. But this is to look ahead, as Herodotus intends us to do, and we must return to the events of the night.

#### IV. THE DAY OF BATTLE

In the small hours Aristides, who in crossing from Aegina had slipped through the Persian ships on patrol,<sup>48</sup> informed the Greek captains at Salamis that they were now encircled by the enemy (Hdt. 8.79-81; Plu. *Them.* 12; *Arist.* 8). Shortly afterwards Panaetius, the captain of a Tenian trireme which had deserted from the Ionian squadron, arrived at the Greek naval base and reported 'the whole truth' (Hdt. 8.82; cf. D.S. 11.17.3 and Plu. *Them.* 12 fin.). Thus, before the night was out, the Greek captains were fully informed of the dispositions and intentions of the opposing fleet. The only point of which they were in ignorance was that Psyttalia was held by Persian troops, its occupation being known presumably to none save the Persian high command. After dawn a ship came in from Aegina, and no doubt brought the news that a Persian squadron had been seen off the western exit of the Bay of Eleusis during the night (Hdt. 8.83.2); but it arrived after Themistocles and Eurybiades had planned the disposition of the Greek fleet.

Themistocles and Eurybiades knew that at dawn the Persian fleet would begin to enter the eastern arm of the Channel and advance towards the Greek naval base. They knew, too, that the Persians expected to take the Greeks by surprise and to find them disunited and ready for flight; for it may be assumed that the desertion of the Tenian trireme during the night was not known to the Persians. It was the intention of Themistocles to lure the enemy onwards, so that the battle might take place in the narrows (Plu. *Them.* 12, ὅπως ἐν τοῖς στενοῖς ναυμαχήσωσιν; cf. D.S. 11.15.4, ἐν ταῖς στενοχωρίαις, and 11.17.1, τὰς περὶ Σαλαμῖνα δυσχωρίας, and Hdt. 8.60 b, τὸ γὰρ ἐν στείνῳ ναυμαχεῖν πρὸς ἡμῶν ἐστὶ). This he could best achieve if, when dawn broke, the Greek fleet appeared to the Persians to be in flight. Moreover, he could calculate that the Persian fleet would require approximately two hours to move from its position at dawn into the narrows between Psyttalia and the Heracleum.<sup>49</sup>

While it was still dark, the Greeks came down to the beach from the town of Salamis (D.S. 11.17.4 fin.: ἀπὸ τῆς Σαλαμῖνος προθύμως συγκατέβαινον εἰς τὴν ναυμαχίαν). As dawn began to break, the marines were already mustered on the shore (Hdt. 8.83: ἥως τε διέφαινε). They were exhorted by Themistocles and other captains, and then they embarked and put to sea. As they headed northwards into the northern arm of the Channel, some of the ships were no doubt visible to the Persian fleet, which was forming into close order off the eastern exit of the Channel. The belief of the Persian command, that the Greeks were in flight, seemed to be confirmed by the sight. In

<sup>46</sup> The word γεφυρώω may mean also 'to dam up' or 'make into a causeway' in the sense that a pontoon-bridge forms a causeway.

<sup>47</sup> Beloch, *Klio* 8, 477-8 and others assume that 'the shore of Artemis' is the Acte of Peiraeus, since there was a sanctuary of Artemis at Munychia (Paus. 1.1.4). They believe, then, that the oracle refers to the midnight position of the Persian fleet when its formation extended from off Cape Cynosoura to Munychia. This interpretation, however, does not accord with Herodotus' understanding of the oracle; for he quotes it not to explain his words at 8.76.1 but to point forward to the battle itself (8.96.2). Beloch's argument that one makes a bridge to connect two points which are opposite to one another is only one aspect of the truth; for in Greek, as in English, one can also speak of bridging a river or a strait (Hdt. 4.88; 118).

<sup>48</sup> So long as the Persian fleet threatened the eastern entry into the Salamis Channel, the quickest crossing from Aegina to the Greek headquarters at Salamis town was the direct one to the western side of the waist of the island, whence it is a short walk or ride to Kamateró. This route must have been much in use while Aegina was held by the main Aeginetan fleet. Aristides will have guessed that the Persian ships off the western

bay moving northwards intended to close the exit from the Bay of Eleusis. This guess was confirmed at dawn by the Aeginetan ship, bearing the images of the Aeacidae, which had come by sea from Aegina (Hdt. 8.83); the only possible route for her was through the Bay of Eleusis. Bury's suggestion in *CR* 10, 418, that Aristides in fact travelled on the Aeginetan vessel which brought the images of the Aeacidae, is contrary to the statements of Herodotus and Plutarch; it entails the consequence that the Greeks were informed first by the Tenian trireme, that Aristides arrived when the Greeks were already embarking at dawn (Hdt. 8.83.2) and that the whole story of Aristides entering the conference of commanders (Hdt. 8.79-81 and Plu. *Arist.* 8, especially οὐδενὸς προειδόντος τὴν κύκλωσιν ἦσαν ὁ Ἀριστίδης) has to be discarded as nonsense. Bury's suggestion encouraged Grundy, p. 391 n., to accuse Herodotus of having mistimed the arrival of the Aeginetan ship and Beloch, *GG* 2<sup>5</sup>, 2, 121 to dismiss Herodotus' account of Aristides' arrival as unhistorical. The choice between them and the ancient authorities is an obvious one. For they, having scrapped Herodotus and Plutarch, have no evidence for their view, but Herodotus no doubt spoke to combatants at Salamis and used contemporary accounts.

<sup>49</sup> For the timing see below, p. 51.



fact, however, when the Greeks were all hidden behind the mass of Mt. Aegaleos, they began to form their order of battle.

The next event is vividly portrayed in the Messenger's speech in the *Persae* 386 f., narrated from the Persian point of view. 'When radiant Day, drawn by her white steeds, took full possession of the land, first of all a cheer rang out from the Greeks, loud like a song, and a shrill echo responded from the island crags. And fear fell on all the barbarians, disappointed of their expectation; for the solemn warsong of the Greeks at that moment was an indication not of flight but of an impetuous onset for battle with bold hearts. The trumpet's blast fired all the scene. Immediately on the word of command they smote the sounding sea with the even stroke of foaming oars, and swiftly they all hove in sight.' As the Persian fleet advanced in full daylight, no Greek ships were to be seen, and the Persians were confident that they had fled. The Persians were disillusioned first by the sound of the war song, then of the trumpet, and finally by the sight of the leading ships, which rowed out into view.<sup>50</sup> The over-confidence of the Persians gave way to fear, and the Greeks gained the initial advantage of surprise.<sup>51</sup>

While it was still unseen by the Persians, we may suppose that the Greek fleet formed into several columns facing south. The leading ships were those of Aegina, the swiftest in the fleet, and after them came those of Sparta and Megara. Next in order came the squadrons of the other allies, and the Athenians under the command of Themistocles held the centre and the rear. While the main fleet was thus drawn up ready for battle, the Corinthian squadron and some other ships were detached from the main fleet and proceeded northward to guard the entry from the Bay of Eleusis (Hdt. 8.94.1).<sup>52</sup> When all was ready, the columns rowed south through the gap between Psyttalia and Heracleum and advanced until the whole formation was in view of the enemy (Aesch. *Pers.* 399-401, θοῶς δὲ πάντες ἦσαν ἐκφανεῖς ἰδεῖν. τὸ δεξιὸν μὲν πρῶτον εὐτάκτως κέρας ἡγεῖτο κόσμῳ, δεῦτερον δ' ὁ πᾶς στόλος ἐπεξεχώρει; D.S. 11.18.2, οὗτοι μὲν οὖν τοῦτον τὸν τρόπον συνταχθέντες ἐξεπλευσαν). Then the ships turned left into line of battle, the Aeginetans on the right wing and the Athenians on the left wing (Hdt. 8.85; D.S. 11.18.1-3). They now held the narrows between Salamis and Heracleum<sup>53</sup> (D.S. 11.18.2 καὶ τὸν πόρον μεταξύ Σαλαμῖνος καὶ Ἡρακλείου κατεῖχον), and faced the oncoming mass of the Persian fleet (Hdt. 8.83.2, ἀναγομένοισι δὲ σφι αὐτῖκα ἐπεκέατο οἱ βάρβαροι).

A period of manoeuvring followed during which the right wing and right centre of the Greek fleet backed water and came close to the shore (Hdt. 8.84.1, οἱ μὲν δὴ ἄλλοι Ἕλληνες [ἐπὶ] πρύμνην ἀνεκρούοντο καὶ ὠκελλον τὰς νέας), that is close to the shore of Salamis, towards which the crews of disabled Greek ships swam during the battle (Hdt. 8.89.1, ἐς τὴν Σαλαμῖνα διένειον). The story which Herodotus relates (8.84.2), that a spirit appeared and called to the whole Greek force 'how long will you go on backing water?', suggests not that the Greeks were dressing their front line but that the operation lasted for some time and that the whole Greek fleet was backing water. Meanwhile the Persian fleet came on, at a further order from the king (D.S. 11.18.3, ὁ δὲ βασιλεὺς τῷ μὲν ναύαρχῳ προσέταξεν ἐπιπλεῖν τοῖς πολεμίοις), and reached the position which fulfilled the oracle of Bacis (Hdt. 8.77), that is to say covering the waters from the Attic coast to the shore of Salamis town and Cape Cynosoura. The positions of the Greek right wing and the Persian left wing were then as shown on Map 2. The Aeginetans on the extreme right had troops on shore behind them; they did not advance into the enclosed water of Ambeláki Bay, which would have given the Persians a chance of breaking the Greek line off Cape Kamateró. But later in the fight, as we shall see, the Aeginetans fought their way out into the vestibule and were able to intercept the enemy vessels in flight (Hdt. 8.91, ὑποστάντες ἐν τῷ πορθμῷ). The Persian left wing now pointed 'east and towards the Peiraeus,' and the Phoenicians facing the Athenian wing, to which we must now turn, pointed 'in the direction of Eleusis and the west' (Hdt. 8.85.1, οὗτοι γὰρ εἶχον τὸ πρὸς Ἐλευσινὸς τε καὶ ἑσπέρης κέρας . . . οὗτοι δ' εἶχον τὸ πρὸς ἡῶ τε καὶ τὸν Πειραιέα).

The precise position of the Athenians, after they had ceased to back water (Hdt. 8.84.2) and before they first engaged, may be deduced from the account in Plutarch, *Themistocles* 14-15. It is there stated that Themistocles did not engage until a swell came up from the open sea into the narrows (τὴν εἰλωθῖαν ὥραν παραγενέσθαι τὸ πνεῦμα λαμπρὸν ἐκ πελάγους αἰεὶ καὶ κύμα διὰ τῶν στενῶν κατάγουσαν). Such a swell from the open sea runs at right-angles to the sides of the Channel and dies out after the bend of the Channel in the waters north of Psyttalia.<sup>54</sup> The Athenian line, therefore, did not extend far north of Psyttalia. Moreover, as the swell rolled the Phoenician vessels off

<sup>50</sup> This essential point, that the Greek fleet was at first out of sight of the Persian fleet and then came swiftly into sight, is not met by many reconstructions of the battle (e.g., J. Keil, plan on p. 333 and other plans in Kromayer-Veith, map facing p. 106).

<sup>51</sup> Whereas Aeschylus mentions the disappearance and the reappearance of the Greek fleet from the Persian point of view, Herodotus omits this step in his narrative of the Greek fleet's preparations. He simply says that the whole Greek fleet was already at sea when the Persians bore down upon them (Hdt. 8.83 fin.). In the same way, whereas Aeschylus *Persae* 374-9 describes the preparations of the Persians on the preceding

evening, Herodotus omits this step and proceeds to midnight, when the Persian fleet was already at sea in formation (Hdt. 8.76.1). In these respects the two accounts happen to be complementary. They do not contradict one another.

<sup>52</sup> See below, p. 49, for a discussion of the Corinthians' task.

<sup>53</sup> See Map 1. This precise description of the Greek position is not taken precisely by many scholars, e.g., by J. Keil, p. 335 n. 2, who says it means only that the Greek fleet lay between Attica and Salamis.

<sup>54</sup> My informant is Mr. Delcayannis; see note 1.



their bearing and exposed their broadsides (Pl. *Them.* 14, ἐσφαλλε προσπίπτον καὶ παρεδίδου πλαγίας τοῖς Ἑλλήσιν ὁξέως προσφερομένοις), the Phoenician ships were heading not up the Channel but more or less across the Channel. For, if they had headed up the Channel, they would have pitched and tossed under oar without losing their bearing.<sup>55</sup> Further, Plutarch narrates that at the time of the first clash the Athenians saw a light shine from Eleusis and a mist spread from the Thriasian plain towards their triremes.<sup>56</sup> Whether in fact there was such a light, may well have been a matter of personal belief; but the story, that the Athenians saw the light and the mist, can only have been circulated if the Athenians were in sight of Eleusis. In other words they were west of a line drawn from Kamateró point to the coast just north-west of Pérama; for anyone east of that line is cut off from sight of Eleusis by the mass of Mt. Aegaleos. Further, as they saw the light from Eleusis, the men on deck are likely not to have had their backs towards Eleusis but to have faced rather towards the east.<sup>57</sup> Lastly, Plutarch *Aristides* 9 states that the hardest fighting and the greatest press of shipping happened around Psyttalia, and that for that reason the trophy of victory was set up on the island. I have therefore placed the Athenian line and the Phoenician line in the positions shown on Map 2.

Xerxes had expected the battle to take place in the narrows. In this expectation he had passed his men unseen on to Psyttalia (Hdt. 8.76.2) and he had placed his throne on the hill above the Heracleum. If the Greek fleet gave way before the Persian fleet, as Sicinnus' message led him to expect, Xerxes wanted to see the northern part as well as the eastern part of the Channel, in fact the whole scene where his forces would be deployed (A. *Pers.* 466-7). This he was able to do, and we may turn to his view of the Persian fleet.

When dawn came, the Persian fleet formed closer order under oar, and it then moved down the Channel. The right wing, held by the Phoenicians, was already in an advanced position and close to the Attic shore. The fleet, therefore, advanced *en echelon*, its right wing leading, and it is probable that the left wing was further delayed by the congestion of shipping which was passing into the Channel in two streams, one between Cynosoura and Atalante and the other on the north side of Atalante.<sup>58</sup> Deceived by the disappearance of the Greek fleet and believing the Greeks to be in flight, the Phoenician squadron was still pressing on towards the narrows when the Greek fleet emerged in column and swung into line to face the *en echelon* formation of the Persian line.

The oracle of Bacis was now about to be fulfilled.<sup>59</sup> As the Phoenicians pressed on towards their objective, the narrows of the Channel, the ships on the Greek right and centre backed water towards the coast of Salamis, and the Athenian ships of the left wing backed water too.<sup>60</sup> My belief is that the latter moved up Channel. Their intention was to draw the enemy into the narrow belt of water, where the long files of ships could not follow in support; for the Persian fleet now advanced into a narrowing wedge of water between the Greek line and the Attic coast, a wedge which was most acute on the Persian right wing, held by the Phoenician squadron. The result was a great congestion. 'The Persians at first preserved their order as they came on, having plenty of room; but when they came to the narrows, they were compelled to withdraw some of their ships from the formation and thereby caused much confusion' (D.S. 11.18.4, οἱ δὲ Πέρσαι τὸ μὲν πρῶτον πλέοντες διετήρουν τὴν τάξιν, ἔχοντες πολλὴν εὐρυχωρίαν· ὥς δὲ εἰς τὸ στενὸν ἦλθον, ἡναγκάζοντο τῶν νεῶν τινὰς ἀπὸ τῆς τάξεως ἀποσπᾶν, καὶ πολὺν ἐποίουν θόρυβον). The confusion was rendered worse when the swell came up the Channel from the open sea. Themistocles then gave the order to attack the rolling broadsides of the Phoenician ships and smash them with the rams of the Greek triremes (Plu. *Them.* 14). And now the Greeks fought in orderly formation, whereas the barbarians were no longer in formation and did not act with intelligence (Hdt. 8.86, τῶν μὲν Ἑλλήνων σὺν κόσμῳ ναυμαχεόντων καὶ κατὰ τάξιν, τῶν δὲ βαρβάρων οὔτε τεταγμένων ἔτι οὔτε σὺν νόῳ ποικιόντων οὐδέν). The vain attempts of the Phoenicians to withdraw to the more open waters (D.S. 11.18.5) only added to the confusion (A. *Pers.* 413 f., πλῆθος ἐν στενῷ νεῶν ἡθροιστο).

The brilliant strategy and tactics of Themistocles need little commentary. He tempted the

<sup>55</sup> The opposing ships were, I take it, facing one another more or less bow on, like sparring boxers, before the swell came up. There was sufficient space between the opposing ships for a ship to make a racing start and gain enough momentum (ὁξέως προσφερομένοις) to use its ram.

<sup>56</sup> Plutarch's account is not to be confused with the story of the dust-cloud in the Thriasian plain (Hdt. 8.65), which was seen by the exiled Athenian, Dicaeus, and by the exiled Spartan king, Demaratus, a day or two before the battle. Many supernatural sights were associated with this campaign.

<sup>57</sup> I take it that the oarsmen's vision was limited to what they could see through the oar-ports and that those on deck would be facing in the direction of the enemy. On my disposition of the Athenian ships some of the oarsmen on the bow side saw Eleusis. That the westward part of the lines of battle were within sight of Eleusis when the engagement began is almost certain from Herodotus' phraseology alone. At 8.76.1, when he describes the position of the Persian fleet at midnight, he mentions simply 'the west wing' (τὸ ἀπ' ἑσπέρας κέρας); at 8.85.1, when he describes the opening of the engagement, he

uses the fuller phrase τὸ πρὸς Ἐλευσινὸς τε καὶ ἑσπέρας κέρας. In this phrase 'pointing in the direction of Eleusis' is no idle addition. It means that at midnight the Persian right wing was not pointing in that direction, but next morning it was. So, too, the Persian left wing was pointing towards Peiraeus and was in sight of Peiraeus. Both phrases become nonsensical if the scene of the battle is supposed to be outside the eastern exit of the channel, as in the plans of Rediades, Munro, Myres, etc.

<sup>58</sup> The retarding of the Persian left wing, which was held by the Ionian squadron, may have lent colour to the view that the Ionians fought backwardly; for in the action they did well (Hdt. 8.85.1).

<sup>59</sup> For the Persian fleet now thronged the Channel from Attica to Cynosoura and to the shore of Artemis by Salamis (cf. p. 45 above).

<sup>60</sup> Goodwin, pp. 257-8, failed to see the object of the Greeks in backing water, and, despite the statement of Herodotus, assumed not a withdrawal but a further advance which carried the Greek fleet out of sight of Eleusis.



Persians, weary after a night at sea, to attack in narrow waters; he gained the advantage of surprise; and he so disposed the Greek fleet that it was able to fight a battle of manoeuvre, use the ram with effect, hold the outer side of a crescent,<sup>61</sup> and rest its right wing and centre on a friendly shore. The Persians, on the other hand, failed to reach their objective, the narrow waters on both sides of the island Psyttalia, on which their troops were already in position. Instead of fighting there a battle of Thermopylae at sea, in which boarding would have been employed, they were engaged in a battle of tactical manoeuvre for ramming and found themselves at a disadvantage.<sup>62</sup>

Aeschylus speaks repeatedly of the deadly effect of the rams of the Greek ships (*Persae* 278-9; 409-11; 415 reading ὑπ' αὐτῶν; 651-2; cf. D.S. 11.18.6). The Greek captains kept their formation on the outer side of the segment, and they left themselves room offshore to manoeuvre into position for ramming. Their ships were heavier, lower in the water, flatter in build, and more handy under oar than the Persian ships with their lofty decks, high sterns, and sluggish movement in the swell (Hdt. 8.60 a; Plu. *Them.* 14). Apart from the initial confusion, into which the Persian formation fell as the waters narrowed, great congestion was caused by the pressing on of the supporting files, whose commanders knew the eye of the Great King was upon them (Hdt. 8.86; 89.2; Plu. *Them.* 15).

The Phoenicians and the Cyprians fared worst; as their leading ships advanced into the narrow waters between the Athenian line and the Attic shore at the bend of the Channel, their supporting files could not come in behind them. Under the repeated charges of the Athenian triremes the Phoenician ships were disabled or driven ashore on the Attic coast below the throne of Xerxes (Hdt. 8.90.1; D.S. 11.19.1-2, πρὸς τὴν γῆν καταδιώξαντες τοὺς Φοίνικας καὶ Κυπρίους).<sup>63</sup> When the leading Phoenician and Cyprian ships were worsted, the Athenians were able to come in from the flank and attack the Cilician, Pamphylian and Lycian ships of the Persian right centre (D.S. 11.19.1-2). The Persian fleet gradually began to break into flight. While the Athenians on the left half of the Greek line dealt with any enemy vessels which ran aground on the Attic coast,<sup>64</sup> and attacked any enemy ships showing fight in the narrows, the Aeginetans on the Greek right wing fought their way forward into the Channel and rammed the enemy ships which sought to escape down the Channel towards the open sea (Hdt. 8.91). The crescent of the Greek line was now changing into a ring as the two wings advanced to enclose and slaughter the enemy (*A. Pers.* 417-8, κύκλῳ πέριξ ἔθεινον). When ships were locked in combat and boarding tactics were employed, the Persian ships held their own or were superior; for they carried many archers and javelinmen, who shot from the high decks, while the Greek ships carried fewer marines (Hdt. 8.90.2; Plu. *Them.* 14). But, where manoeuvre was possible, the ram of the Greek ships was decisive and the Persian marines were ineffective (*A. Pers.* 278-9: οὐδὲν γὰρ ἤρκει τόξα, πᾶς δ' ἀπώλλυτο στρατὸς δαμασθεὶς ναίοισιν ἐμβολαῖς). The final scene of confusion, in which the Persian seamen were speared like tunnies in the water until the fall of night, is vividly described by Aeschylus (*Pers.* 417-28).

During the mêlée off Salamis (Hdt. 8.95, ἐν τῷ θορύβῳ τούτῳ τῷ περὶ Σαλαμίνα γενομένῳ) Aristides observed the Persian infantry on the island of Psyttalia. Raising an assault force from the Athenian cleruchs of Salamis, who were lining the shore below the town, he crossed in small boats to the island, which, lying between the Greek line and the coast of Salamis, was completely surrounded (*A. Pers.* 457, ἀμφὶ δὲ κυκλοῦντο πᾶσαν νῆσον). The total destruction of the isolated Persian force was witnessed by Xerxes, sitting upon his throne above the Heracleum (Hdt. 8.95; *A. Pers.* 447-71; Plu. *Arist.* 9).

During the closing phase of the battle a westerly wind sprang up and favoured the withdrawal of the Persian ships, which hoisted sail to run downwind for the open sea (*A. Pers.* 480-1, κατ' οὐρον; Hdt. 8.96.2, ἀνεμος ζέφυρος). During the evening and the night the crippled and abandoned hulls, which, being wooden and stripped for action, did not sink but floated with their bulwarks or sides awash (*A. Pers.* 418-19), were driven downwind past Phalerum as far as the rocky shore known as 'Colias'.<sup>65</sup> The Greeks were able to collect only those wrecks which were still drifting in the

<sup>61</sup> The importance of such a position is clear from the action off Artemisium (Hdt. 8.16).

<sup>62</sup> The best appreciation of Themistocles' genius is given by Thucydides, who saw that Themistocles was responsible for the battle being fought ἐν τῷ στενῷ and that this saved the situation (1.74.1). In his strategy Themistocles showed the qualities for which Thucydides praised him (1.138.3): the finest judgement in immediate situations which admitted of least deliberation, the most accurate insight into the future development of events, and the ability to improvise the necessary measures.

<sup>63</sup> The Phoenician captains had a short climb to Xerxes' throne, where they were promptly decapitated (Hdt. 8.90.1-3).

<sup>64</sup> Or on the reef of rocks off Pérara to which Timotheus, *Persae*, 97 f. may refer: φυγῇ δὲ πάλιν ἴστο Πέρσης στρατὸς βάρβαρος ἐπισπείρον. ἄλλας δ' ἄλλαν θραύεν σῦρτις. So, too, *A. Pers.* 421 ὅκται δὲ νεκρῶν χοιράδες τ' ἐπλήθουν.

<sup>65</sup> Honigsmann in P.-W. *RE* 11, 1, col. 1077, discusses the

difficult problem of the identification of Κωλιάς ἕκρη. Strabo (9.1.21, C 398) places the temple of Aphrodite Colias not far from Cape Sunium near Anaphlystus (now probably Anávyso) and adds that this was the spot to which the last wreckage of the Persian fleet after the battle was cast up by the waves (εἰς ἐν τόπον ἐκκουμανθῆναι τὰ τελευταῖα τὰ ἐκ τῆς περὶ Σαλαμίνα ναυμαχίας τῆς Περσικῆς ναυαγία φασί). I take it that Strabo means that the wreckage extended along the coast as far as Cape Colias (τὰ τελευταῖα . . . ναυαγία), and that a heavy sea running on to the coast, which had been raised by a southerly wind in the Aegean Sea, washed the wreckage up on that stretch of coast. Honigsmann rejects Strabo's placing of the temple of Aphrodite Colias and prefers to put Cape Colias at the Cape of Áyios Yeóryios just east of Phalerum Bay (following Milchhöfer, 2, 2). Although it makes little difference to my argument, I think that Strabo's evidence is too strong to be rejected in this manner and that Cape Colias should be placed at Cape Anávyso.



Channel close to the town of Salamis (Hdt. 8.96). On the next day the Greeks prepared for a further engagement. Xerxes, however, reverted to his first plan, the construction of a mole to Psytalia and Kamateró; but, as the season was far advanced and storms were to be expected, he soon abandoned this plan and set off with his fleet and part of his army for the Hellespont.

A few points in the battle remain to be clarified. Herodotus tells an Athenian story, which he himself evidently did not believe to be true, namely that at the beginning of the engagement the Corinthians under Adeimantus' command hoisted sail, fled as far as the temple of Athena Scirias, and returned when the action was over (8.94). The epitaphs of Adeimantus and the Corinthians, the evidence of Herodotus' contemporaries, and the arguments of modern scholars condemn the Athenian story as false. But, in order to be plausible, it must have been based on the movements of the Corinthian squadron. The Corinthians' task was presumably to cover the rear of the Greek fleet against attack from the Persian squadron, which during the night, as Aristides and the Aeginetan ship reported, had cut the Greek line of retreat. At dawn the size of this Persian squadron and its position at sea were not known to the Greek commanders. They could argue with probability that the Persian squadron would outnumber the seventy Greek ships which accompanied Adeimantus;<sup>66</sup> and further that it was either in position at the narrows off Nisaea or else proceeding through the Bay of Eleusis. Adeimantus, therefore, hoisted sail to make all speed to the northern exit of the Salamis Channel, whence he could see the Bay of Eleusis. There, no doubt, he waited; for he had no wish to fight a probably superior Persian squadron in the open waters of the Bay, and his best position for battle was in the narrows of the northern exit of the Salamis Channel. The temple of Athena Scirias is unidentified, but Herodotus placed it in the territory of Salamis, and Plutarch placed it at the borders of the territory of Salamis (Hdt. 8.94.2; Plu. *Mor.* 870 b, *περὶ τὰ λήγοντα τῆς Σαλαμίνος*). If, as is the natural supposition, the territory of Salamis town is meant, the site may well be near Cape Arápis.<sup>67</sup> When no danger threatened from the Bay of Eleusis, the Corinthians returned and joined in the battle; they came up behind the victorious Athenian wing, and their participation could later be denied by the Athenians, who were the chief eyewitnesses of what took place.

The Persian squadron, which sailed at once after the receipt of Sicinnus' message to cut the Greek line of retreat, probably numbered 200 ships drawn from the Egyptian contingent. Its task was to block the narrows between the island of Salamis and the territory of Megara (D.S. 11.17.2, *τὸν μεταξύ πόρον τῆς τε Σαλαμίνος καὶ τῆς Μεγαρίδος χώρας*; Plu. *Them.* 12). Its commanders believed that the Greek fleet would take flight in the night or early morning. They, therefore, had no intention of encountering the larger Greek fleet in the open waters of the Bay of Eleusis. No doubt they remained all day where they had been ordered to remain, at the narrows off Nisaea, and they took no part in the battle.<sup>68</sup>

Some indications of the weather are given in the sources. When the main Persian fleet kept its position during the night between the island and Munychia, it probably did so under oar (A. *Pers.* 376-83). In any case there was during the night no strong wind or heavy sea such as would have prevented the Persian fleet from holding station in these open waters. At dawn, however, a southerly wind was blowing; for the Corinthians sailed before it towards the north (Hdt. 8.94.1).<sup>69</sup> But the wind was not yet fresh (Plu. *Them.* 14). The sea was calm enough in the Channel of Salamis for the Persian fleet to advance under oar and for the Greeks to muster and advance under oar. Some hours after dawn, perhaps about 9 a.m., a fresh wind blew and the swell from the open sea came up the Channel (Plu. *Them.* 14, *τὸ πνεῦμα λαμπρὸν ἐκ πελάγους αἶε καὶ κύμα διὰ τῶν στενῶν κατάγουσαν*). Now the only wind which brings a swell 'from the sea through the straits' is a south wind. Such a swell is generated far out at sea. In the Channel it may or may not be accompanied by a south wind. An experienced sailor, who knew the waters of Salamis well, in-

<sup>66</sup> The figure of seventy ships is obtained by subtracting Aeschylus' figure of 310 Greek ships at the opening of the action from Herodotus' total of 380 ships at the concentration of the fleet at Salamis. The Corinthians had forty ships, and their colonists at Ambracia and Leucas had ten ships, which probably acted with the Corinthians. Among the remaining twenty ships it is probable that six Spartan ships under Eteoniceus and some Aeginetan ships were included (Lycurg. *c. Leocr.* 70; for the number of Spartan ships see Hdt. 8.43 and Isoc. *Panath.* 50). This point is excellently made by Labarbe, pp. 434 f. I differ from him in my view that the Corinthian force did not engage the Egyptian squadron but returned to take part in the main action; the words of Hdt. 8.94.4, *ἐν πρώτοις σφίσις αὐτοῦς τῆς ναυμαχίας ναυίζουσι γενέσθαι*, which Labarbe seems to take in a temporal sense, are to my mind qualitative and mean that the Corinthians considered themselves to be among the most distinguished fighters.

<sup>67</sup> Cf. Büchner and Kock in P-W., *RE*, 1, 2, col. 1829, and 3, 1, col. 534. If the island Nerá was in antiquity a cape of Salamis, it is a possible site for the temple.

<sup>68</sup> Their absence has often been noted. Herodotus refers to the Egyptians in two speeches only, where the King's subjects

are enumerated as a worthless lot (8.68.7; 8.100.4).

<sup>69</sup> In summer the normal wind of the early morning breathes off shore into the Bay of Eleusis (so also Grundy, p. 398 note), and comes down the Salamis Channel as a north-westerly or westerly breeze, raising no sea, but adverse to anyone sailing up the Channel. On this day in September the weather was not normal; for the Corinthians hoisted sail to proceed up the Channel, and, as a trireme under sail was aided only by a following wind, it is clear that the wind was southerly at dawn. About 9 a.m. (on my approximate reckoning) an offshore wind was breathing offshore into the Bay of Eleusis and carried some mist from the Eleusinian plain towards the Channel (Plu. *Them.* 15), where it settled suddenly. It is probable that the weather was fluky in the Channel, because a southerly wind had been, and perhaps still was, blowing hard out at sea. Such conditions were to be expected in September when the Battle of Salamis was fought. Compare the proverb of the Middle Ages quoted by Rodgers, p. 85, 'in the Mediterranean there were only four good ports for a fleet, namely June, July, August and Port Mahon', and note the storms which befell the Persian fleet in August.



formed me that one can tell from the cloud formation that a strong south wind is blowing out at sea and in several hours' time, even twelve hours' time, will bring a swell into the Channel.<sup>70</sup> Plutarch thought that Themistocles knew; for according to him Themistocles held the Greek ships off, until the swell came (Plu. *Them.* 14). He might have known on the evening before the battle, or he might have judged the case at dawn when a southerly wind was already blowing over Salamis. Such a south or south-easterly wind, now called the 'Sirocco', is common in September.

The Sirocco is usually followed at an interval of some hours by a westerly (to north-westerly) wind or by a north-westerly (to north) wind, called respectively the Maistro Bounentis and the Maistro; both are wont to blow very strongly for one or two hours before sunset. The Maistro Bounentis blows driftwood in the Salamis Channel on to the coast north of the Peiraeus. The Maistro blows such driftwood out past the Peiraeus into the Saronic Gulf. Either wind would suit the words of Herodotus ἀνεμος ζέφυρος (8.96.2) and of Aeschylus κατ' οὐρον (*Pers.* 480-1), but the casting up of wreckage on Cape Colias can only have been due to two facts, the blowing of a Maistro wind and the running of a sea, raised earlier by a Sirocco, which carried the wreckage on to the coast east of Phalerum. The Maistro probably blew itself out after sunset, and next morning the weather was quiet enough for the Greeks to expect another attack and for Xerxes to resume operations on the construction of the mole (Hdt. 8.97; 108).<sup>71</sup>

The area required for the deployment of the two fleets in their battle-order can be roughly calculated. The dimensions of the Greek trireme in the latter half of the fifth century B.C. were up to 110 feet long by 15 feet wide. At Artemisium one Athenian trireme carried 200 men (Hdt. 8.17), and at Salamis the 180 Athenian triremes carried eighteen marines each (Plu. *Them.* 14).<sup>72</sup> As these triremes were heavy in build (Hdt. 8.60 a) and the later art of swift manoeuvre for ramming was not yet fully developed, it is probable that the triremes at Salamis were at least as large as the largest trireme of the latter half of the fifth century B.C.<sup>73</sup> Under oar at sea such a trireme covered approximately 110 feet by 35 feet.<sup>74</sup> The interval between triremes in column in calm water may be assessed at 100 feet and between triremes in line at 50 feet,<sup>75</sup> in order that there should not be a loss of manoeuvrability. On this basis a column consisted of some twenty-five triremes per mile, and a line consisted of some sixty triremes per mile.

The Persian ships were in general lighter in build, but they had lofty decks and high sterns and a normal complement of 200 men to a ship (Hdt. 7.184.1; 8.60 a; Plu. *Them.* 14); at Artemisium the Persian ships carried an additional thirty marines (Hdt. 7.184.2), and at Salamis they may have done likewise. In the water the dimensions of the Persian ships (excepting the large flagships) were probably much the same as those of the Greek triremes. Being lighter in build and carrying more superstructure, they were faster under sail and perhaps under oar (Hdt. 8.10.1); but they were certainly less handy under oar when a swell was running (Plu. *Them.* 14). When the Persian fleet entered the Channel of Salamis, it was 'in close array' (A. *Pers.* 366), and there proved to be so many ships in the narrows that, just before the engagement developed, some ships had to be withdrawn and confusion resulted (D.S. 18.4; Hdt. 8.86). I therefore propose to put eighty ships per mile into the Persian line and thirty ships per mile into a column, the intervals of clear water being respectively 30 feet and 66 feet.<sup>76</sup> This close order was designed for a battle of boarding on a calm sea. For in the enclosed Channel of Salamis local winds raise only small waves, and the Persian command did not foresee, as Themistocles may have done, the arrival of the swell from the open sea.

When the Greek fleet of 310 ships came out from the northern arm of the Channel, it may have rowed in ten columns, each led by one of the ten fast sailers (A. *Pers.* 340), which, according to the scholia, were Aeginetan ships. The ten leading ships covered a frontage of less than 300 yards of water. The depth of the formation was a mile and a quarter. When the formation cleared the waters by Psyttalia, the columns swung left into line, and the 310 ships probably closed up to form four lines of some seventy-five triremes each. If we allow 50 feet of clear water between ship and ship, the front line was about one and a quarter miles long. Its length corresponded to the depth of the column-formation. The Aeginetan column-leaders were now on the right wing of the line-formation. If we allow 100 feet of clear water between line and line, the four lines in depth covered some

<sup>70</sup> My informant is Mr. Deleyannis (see n. 1). The only local wind which creates a sea in the Salamis Channel is the strong north-westerly wind, common in winter, which makes the passage from Péräma to Kamateró difficult. In the ancient evidence about the winds on the day of the battle the only problem is contained in the words of Plutarch (*Them.* 14) that Themistocles waited for τῆς σταθίας ὥραν to bring up the strong wind from the sea and the swell. He may refer to the fact that the wind normally is strong about 9 a.m., but it does not apply necessarily to a southerly wind of the type which brings up the swell from the open sea.

<sup>71</sup> For the position of Cape Colias see n. 65. I obtained this information about wind and drift from Mr. Deleyannis. The *Mediterranean Pilot*, 4, 111, comments on the strength of the Maistro.

<sup>72</sup> Grundy, p. 399, says the Greek ships carried thirty-six marines, which seems to rest on no ancient evidence.

<sup>73</sup> Rodgers, p. 55, misled perhaps by modern analogies, believes that the early trireme was less large than the later model, but he forgets the change in tactics and its effect on ship-construction in the second half of the fifth century B.C.

<sup>74</sup> Rodgers, p. 30, puts the longest oar for one oarsman at 14 feet and demonstrates that almost one-third of the oar must be inboard of the thole-pin. Assuming that no outrigger was used, since it would be vulnerable in boarding and does not appear in vase-paintings, etc., of triremes, I conclude the width of a trireme under oar was 35 feet amidships. Some scholars reckon 45 feet, e.g., Myres, p. 281.

<sup>75</sup> Grundy, p. 396, allows 60 feet per ship, that is an interval between ship and ship of only 25 feet, which seems to be too small for the ships to be able to manoeuvre freely.

<sup>76</sup> Rodgers, pp. 89-90, allows 25 feet and 45 feet of open water in line and in column respectively for the Persian fleet during its advance.



230 yards of water. It was essential for the Greeks that their line should not be pierced; for the superior number of ships in the Persian supporting files could exploit any gap, pass through, and take the Greeks in the rear. A depth of four lines in the Greek fleet seems to be reasonable, and at the same time the formation had to be in a sufficiently open order to permit of manoeuvre.

When the Persian fleet of 1,207 ships was under oar during the night it must have been in open order, say twenty ships per mile in line and ten ships per mile in column. In the reconstruction which I have suggested, the front line of the Persian fleet was some 4 miles long and contained about eighty ships, and the depth of the whole formation was about one and a half miles; it thus covered the waters as far eastward as the coast of Munychia (Hdt. 8.76.1). At dawn the fleet formed into close order, before it advanced into the Channel. Its front may have been a little more than one mile at some ninety ships and its depth some thirteen ships, covering about half a mile. As the Persian array moved up the Channel, the *en échelon* slant enabled the front to lengthen and perhaps 100 ships to form the front line. Even so, the bottleneck for the entry of the supporting columns remained at the mouth of the Channel between the tip of Cape Cynosoura and the Attic coast. When the Persians engaged the Greeks, the Persian line was almost as long as the Greek line, but it was probably in less regular order (*cf.* Plu. *Them.* 15, οἱ δ' ἄλλοι τοῖς βαρβάροις ἐξισούμενοι τὸ πλῆθος, ἐν στενῷ κατὰ μέρος προσφερομένους καὶ περιπίπτοντας ἀλλήλοις); for the Persians had to overcome four difficulties, the narrowing width of the Channel as they approached the Heracleum, the deployment of their left wing and centre to face the Greeks, the extension of their right wing in pursuit of the Athenians, and the congestion of the supporting files at the bottleneck. Because of these difficulties and the forehandedness of the Greek commanders I have placed the Greek line on the outer side of a crescent formation, which gave them more sea room than their opponents. During the action both Greek wings advanced to enclose the Persians in an almost semicircular formation (Hdt. 8.91-2; A. *Pers.* 417-18), and their formation probably contracted in consequence. The Persian loss of more than 200 ships (D.S. 11.18.3) suggests that at least three or four lines of Persian ships were deeply engaged in the action and that perhaps two-thirds of their fleet fled before they became embroiled.

The timing of the battle may be calculated approximately. In the preparations for the battle an average speed of 2 knots under oar is reasonable.<sup>77</sup> The Persian ships at the rear of the formation, some one and a half miles in depth, closed up to within half a mile of the leading ships; the move over a mile may have taken half an hour at the minimum. The Greek ships at Kamateró had to go a mile or so to their assembly-point, for which we may allow half an hour at the minimum. The Persian formation then advanced in close order for some 2 miles, for which we may allow one hour at the minimum. The Greek formation rowed swiftly out in column (A. *Pers.* 398), covering a rather shorter distance at perhaps twice the speed, say in half an hour. The Greek ships were thus in line perhaps half an hour before the Persian ships came up close. In practice, I imagine the assembling of both sides was less rapid, since the numbers were so large. I should be inclined to reckon from dawn at 6 a.m. at least two and a half hours for the Persians and two hours for the Greeks to reach their battle positions (see Map 1). A period of manoeuvring then ensued, during which the Greek ships backed water slowly. We may allow half an hour or more for this operation, so that the swell came up the Channel about 9 a.m. and the action then started.

The timing of the movements made during the night can be estimated approximately if we assume that the ships travelled at some three and a half miles an hour, *i.e.*, about 3 knots.<sup>78</sup> The Aeginetan ship, which arrived at dawn at Kamateró, had travelled almost 30 miles from Aegina in some eight and a half hours; taking dawn at 6 a.m., we conclude that she left Aegina at 9.30 p.m. and reached the narrows off Nisaea at about 2.30 a.m. Aristides reached the conference of the Greek commanders some time before dawn, say about 4 a.m. If he took an hour to cross the waist of the island,<sup>79</sup> he landed at 3 a.m., having covered 19 miles by sea in some five and a half hours; he left Aegina, then, about 9.30 p.m. and turned Cape Petritis to enter the western bay of Salamis at about 1.30 a.m. As the Egyptian squadron was seen by Aristides at 1.30 a.m. and by the Aeginetan ship some 3 miles later at 2.30 a.m., it follows that the Egyptians left Phalerum Bay about 8.0 p.m. and covered their 23 miles in six and a half hours. Before the Egyptians started, Xerxes had held his conference on hearing the message of Sicinnus, and the Egyptians on receiving their order from Xerxes had no doubt taken on supplies for a minimum of 48 hours. Let us deduce, then, that Sicinnus gave his message about 6 p.m. when dark was falling.

Having received the message and conferred with his commanders, Xerxes sent off the Egyptian squadron at the earliest moment (Plu. *Them.* 12 and *Arist.* 8, νύκτωρ ἀναχθεῖσαι). If it was a moonlit night, the moon being in the east would not help any Greek observers on Cynosoura to see the Egyptians.<sup>80</sup> Xerxes could calculate that the Egyptian squadron would close the strait off Nisaea about 2.30 a.m. As the distance from Kamateró to the strait off Nisaea is some 12 miles, Xerxes

<sup>77</sup> Rodgers, p. 87, 'certainly the speed was not over 2 miles an hour'.

<sup>78</sup> Rodgers, p. 53, gives examples of trireme speeds. The night was calm; for the main Persian fleet was under oar during the night.

<sup>79</sup> See note 48 above. I am assuming that he went on a ship as fast as a trireme; if he went on a slower vessel, he left before the Aeginetan ship and still arrived well before dawn.

<sup>80</sup> See note 41 above.



would effectively bar the western route of escape from 11.30 p.m.; for any Greek ships, which fled after 11.30 p.m. even at a speed of 4 miles an hour, would reach the strait off Nisaea at 2.30 a.m. or later. It was important for Xerxes not to put the Greek fleet in motion before 11.30 p.m., because if, as he believed, many intended to take flight, they would get away before the Egyptian squadron was in position. He delayed, therefore, until midnight, before he brought his fleet forward to the waters near the eastern exit. By that time, too, the moon was rising in the south, and his fleet, operating in silence, was not observed by the Greeks, except perhaps in the vicinity of Ceos.<sup>81</sup>

We do not know when Xerxes landed his Persians on Psyttalia. It is not clear whether Herodotus is describing the operations in chronological order (8.76.1), and one supposes Xerxes would have waited until midnight, in case the landing was detected and the Greeks fled towards the western exit. The landing itself was not particularly hazardous; for, if the island was already occupied by the Greeks,<sup>82</sup> the landing party could have been withdrawn without much difficulty. In fact, it was not already occupied by the Greeks, and there was plenty of scrub to provide concealment for the Persians at dawn.

The failure of the Greeks to occupy Psyttalia may seem surprising. However, if we bear in mind the divided counsels of the Greek commanders and the wish of the majority to withdraw towards the Isthmus, we can understand the reluctance of any one commander to put his troops on an island which was likely to be abandoned by the Greek fleet during the night. The island, too, had no water and provided no facilities for beaching triremes. Next morning, when the Greeks decided to fight, their plan was to engage the enemy on the far side of Psyttalia, and there was no point then in occupying it.

Xerxes' purpose in occupying Psyttalia is explained by Herodotus (8.76.2; cf. *A. Pers.* 450-3). If the Greek fleet or a part of it showed fight, the battle would be in the narrows, where his men on Psyttalia could help their friends and attack their enemies. Xerxes probably had a further purpose in mind. If the Greek fleet did not stand its ground but fled, it would fight off the narrows by Nisaea.<sup>83</sup> In any case, wherever it fought, Xerxes expected that it would be defeated and the ships and men would take refuge on Salamis island; again, if it did not fight at all, it would still hold Salamis island. The Greeks, indeed, would be in the position of defending Salamis, as some of them had feared (Hdt. 8.49.2, ἐν Σαλαμῖνι μὲν ἔοντες πολιορκήσονται ἐν νήσῳ ἵνα σφί τιμωρία οὐδεμία ἐπιφανήσεται). But to capture the fleet, the crews, and the marines, totalling more than 75,000 men, was no easy matter. A sea-borne landing from comparatively small ships without any covering fire from artillery in the face of so great a force was almost impossible.<sup>84</sup> The best method was the one which Xerxes had already begun and abandoned, namely to complete the mole as soon as he had control at sea. For this task Psyttalia was of the utmost importance. Xerxes, therefore, occupied Psyttalia in anticipation of his next move. From Psyttalia he would have little difficulty in making a causeway across the shallow water to one or more points on the shores of Kamateró and Paloukia. Once a bridgehead was established with the aid of his archers, he could bring his army into the island (Ctesias fr. 26, περὶ τῆς αὐτῆς διαβήναι διανοοῦμενος) and complete the destruction of the Greek fleet, ships and crews alike. The plans of Xerxes were excellent except in the one respect that he was duped by Themistocles.

## V. EPILOGUE

Now that the evidence for the battle has been set out, it is appropriate to reflect on the evidence pertaining to Salamis and Psyttalia. One trophy was set up in the town of Salamis (Paus. 1.36.1), and another was set up on Psyttalia (Plu. *Arist.* 9), which was the island in front of the town of Salamis (Paus. 1.36.2). The sites for the trophies were no doubt chosen because the majority of the Persian ships were destroyed by Salamis (Hdt. 8.86, τὸ δὲ πλεῖθος τῶν νεῶν ἐν τῇ Σαλαμῖνι ἐκεραΐζετο), the wrecks were towed into Salamis (Hdt. 8.96.1, κατειρύσαντες ἐς τὴν Σαλαμῖνα), the turmoil of battle was off Salamis (Hdt. 8.95, ἐν τῷ θορύβῳ τούτῳ περὶ Σαλαμῖνα), the scene of the conflict developed from the Greek position in the strait between Salamis and the Heracleum (D.S. 11.18.2, τὸν πόρον μεταξύ Σαλαμῖνος καὶ Ἡρακλείου), and the greatest press of ships and the severest fighting were in the vicinity of Psyttalia (Plu. *Arist.* 9, ὁ γὰρ πλεῖστος ὠθισμός τῶν νεῶν καὶ τῆς μάχης τὸ καρτερώτατον ἔοικε περὶ τὸν τόπον ἐκεῖνον γενέσθαι διὸ καὶ τρόπαιον ἔστηκεν ἐν τῇ Ψυτταλείᾳ), an island which was admirably described by Aeschylus (*Pers.* 447), Pausanias (1.36.2; cf. 4.36.6), and Plutarch (*Arist.* 9) as lying in front of Salamis and by Herodotus as lying

<sup>81</sup> See note 41 above.

<sup>82</sup> Xerxes' men, who held the area by the Heracleum, had no doubt observed during the day that Psyttalia was not occupied.

<sup>83</sup> Xerxes probably had a throne prepared for him on Mt. Kérata near these narrows, so that he could watch a battle there; this makes sense of Acetodorus' statement which Plutarch, *Them.* 13, records without explanation. There are no grounds for supposing, as Rediades does, that a spur of Mt. Aegaleos was called Kérata and that Acetodorus transferred it to the borders of Attica and Megaris.

<sup>84</sup> The difficulty of such a landing is clear from Thucydides 4.10.5. The same difficulty caused Datis and Artaphernes to land their army at Marathon, where they were unopposed, and not to attempt a landing at Phalerum Bay after the battle when they were opposed. The alternative policy of blockade, which was suggested in the speech attributed to Artemisia (Hdt. 8.68), would have been difficult to execute during the bad weather of autumn; for the coast-line of Salamis is a long one, and the example of Sphacteria is illuminating.



between Salamis and the mainland (8.76.1). Even the oracle of Bacis (Hdt. 8.77) adds a clue; for the Persian ships extended, like a bridge over the waters, to the sacred shore of Artemis, whose shrine was built in the town of Salamis (Paus. 1.36.1), and to sea-washed Cynosoura. If all this evidence<sup>85</sup> is accepted, and the scene of the Battle is fixed in the narrows of the Channel of Salamis near Áyios Yeóryios, Pérama, and Kamateró, the accounts given by Aeschylus, Herodotus, Diodorus, and Plutarch make sense.<sup>86</sup>

If Psyttalia is identified with Lipsokoutáli and the references to Salamis are all taken to refer not to the town but to the island of Salamis, we let loose a veritable *fons errorum*. I cite as an example the reconstruction of the Battle by Grundy, who seems to me to have written more convincingly than most adherents of this identification. Psyttalia being Lipsokoutáli, the Battle was fought somewhere near the eastern mouth of the Channel (see his map facing p. 384 in *The Great Persian War*). As these were the waters which the Persians approached on the day before the Battle according to Herodotus 8.70 (ἀνῆγον τὰς νέας ἐπὶ τὴν Σαλαμῖνα), Grundy asserts that Herodotus 'antedated a movement made on the night preceding the battle to the previous afternoon' (p. 373). As Aeschylus states that the Greek fleet in forming its order of battle was invisible to the Persian fleet, Grundy has to keep the Persian fleet south of Lipsokoutáli. Therefore he cannot allow the Persian fleet to have swung its western wing towards Salamis (Hdt. 8.76.1) at midnight. He asserts, then, that Herodotus 'further antedated the movements in the battle itself to the night preceding the battle' (p. 373); that 'as a description of the movements made in the night by the Persian fleet, this passage in Herodotus is wholly mistaken' (p. 386); and that when Herodotus speaks of the 'west wing' at 8.76.1 Herodotus really means Grundy's east wing, which only became Grundy's west wing in the course of the battle (p. 386). As it is impossible for Aristides and his force to attack Psyttalia = Lipsokoutáli during the battle, since the island lay behind the Persian lines, he asserts that the attack took place after the Persian flight was completed (p. 403), which means that Herodotus and Plutarch were mistaken in their timing of the event.<sup>87</sup> This catalogue of errors by Herodotus is sufficient to damn the reputation of any historian, not only in the eyes of us moderns but also, more important, in the eyes of those participants in the battle who heard or read Herodotus' history in their lifetime. I prefer to take the simple view that Aeschylus, Herodotus, and others made good use of good evidence and that the errors are to be found not in their accounts but in the modern interpretations of those accounts.

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## VI. APPENDIX ON THE NEW SCHOLIA TO THE *PERSAE*

The Scholia of Demetrius Triclinius to the *Persae* were published for the first time in 1948 by L. Massa Positano in *Coll. di studi greci*, 13. They contain a little new evidence on matters of topography.

On *Persae* 303 (Artembares), στύφλους παρ' ἀκτὰς θείνεται Σιληνιδῶν, Positano f 92 r publishes the scholium: Σιληνία αἰγιαλός ἐστι Σαλαμῖνος οὕτω καλούμενος· λέγεται δὲ καὶ Τροπαία ἄκρα ὡς Φιλόξενος ἐν τῷ περὶ λιμένων ἱστορεῖ. This confirms the original text of the Scholium Codicis M, Σιληνία αἰγιαλός Σαλαμῖνος τῆς λεγομένης Τροπαίου ἄκρας ὡς Τιμόξενος ἐν τῷ σ' περὶ λιμένων, into which πλῆσιον had been inserted between Σαλαμῖνος and τῆς by Blomfield, Dindorf, and

<sup>85</sup> In this article I have not discussed the identification of the Κύχρεος πάγος for which Lolling, p. 8, and Milchhöfer, 7.28, give the references, as they shed little light on its position and do not affect the location of the battle.

<sup>86</sup> The only point not yet mentioned which may be regarded as inconsistent is the statement in Diodorus 11.17.3 that the Ionian commanders sent a Samian swimmer to inform the Greeks of Xerxes' plans and of their own intention to desert. This statement was probably derived through Ephorus from an early source. For Herodotus remarks that only a few of the Ionians fought backwardly (8.85.1), and he discounts the charge made by the Phoenicians that the Ionians betrayed the fleet (8.90.1). In these passages Herodotus was probably refuting such a claim as appears in the narrative of Diodorus. Whether an individual Samian swam across during this eventful night, there is no means of determining. The mention of a Tenedian instead of a Tenian trireme (Plu. *Them.* 12) is presumably a slip by him or by his copyist.

<sup>87</sup> To catalogue the errors attributed by modern scholars to the ancient writers, and Herodotus in particular, would serve little purpose, and a few examples may suffice. Grundy further believes that Herodotus forgot 'the enormity of the losses he represents the Persians to have suffered in the two storms' (p. 374), that Herodotus made a mistake as to the time and means of Aristides' arrival (p. 390), and that Herodotus and Diodorus both erred in not placing the Athenians and the Aeginetans next to one another in the battle (p. 400).

Munro, *JHS* 22, 326-7 believes that Herodotus 'holds a brief

to vindicate the veracity of Bacis and tries to force the situation at Salamis into conformity with his oracle', 'Herodotus' conception is hopelessly irreconcilable with the descriptions of Aeschylus', 'Herodotus would seem to have antedated the start from Phalerum to the afternoon and postdated the envelopment to midnight', 'Herodotus has simply adapted or misinterpreted his information to suit his preconceived idea of the battle', and both Aeschylus and Herodotus were mistaken in supposing Psyttalia to have been occupied for the purpose of assisting in the naval battle. In *CAH* 4, 304 f. and map 9, Munro adds to this list a spoken or tacit rejection of Herodotus' and Diodorus' descriptions of the Greek order of battle by putting the Athenians on the right wing pointing towards Salamis and not Eleusis and the Aeginetans, etc., on the left wing facing south, so that the Greek seamen would have had to swim through the press of ships to reach Salamis (Hdt. 8.89.1) and the Phoenicians would have been pushed through the Aeginetan ships to reach land near Munro's site for Xerxes' throne (Hdt. 8.90; D.S. 11.9.2). To attribute to Herodotus 'a childish misconception of the battle' in this wholesale manner is justifiable if one regards Herodotus as a man of childish intelligence in these matters and if one assumes that the conception of the battle in Herodotus' pages comes entirely from Herodotus; but it is most unlikely that Herodotus failed to consult the earlier accounts of the battle and to learn from participants in the battle, and these cannot all have been childishly misconceived.



Meineke; it is now apparent that Dähnhardt was correct in rejecting the insertion of *πλησίον* as mistaken. The beach *Sileniae* is in both scholia a shore of Cape Tropaea. As the Cape received its name from the trophy and from the cult of Zeus Tropaios (*IG* 2.467.27), which was established to commemorate the victory, it is clear that the Cape was adjacent to the waters in which the battle was fought. If therefore we can fix the position of Cape Tropaea with its own beach *Sileniae*, we shall fix the scene of the battle more precisely.

The corpse of Artembares was still awash off this rough beach after the battle when a westerly wind had blown many of the wrecks and corpses to the Attic shore at Colias (*Hdt.* 8.96.2; cf. *A. Pers.* 481). If the battle had been fought outside the Salamis Channel in the waters east of Cape Cynosoura, all the wrecks and corpses, including that of Artembares, would have been cast up on the Attic coast or else driven out to sea. If, however, the battle was fought inside the Salamis Channel as I have argued, some of the wrecks and corpses would have drifted on to the shores of Cape Kamateró and the north shore of Cape Varvári. When we turn to the Scholia for a closer location of *Sileniae* and Cape Tropaea we are faced with the old problem, whether *Σαλαμίνος* means the town or the island of Salamis. In this case there are strong indications that the town Salamis is meant. In other sources two trophies are mentioned, one on Psyttalia and one in the town of Salamis (*Paus.* 1.36.1). It is most unlikely that there was a third trophy; in other words, the trophy in the town of Salamis gave its name to the town's own promontory, Cape Tropaea. We now understand the exact meaning of the Scholium Codicis M, *Σιληνίαι αἰγιαλὸς Σαλαμίνος τῆς λεγομένης Τροπαίου ἀκρῶς*. Further, the source of the information is Philoxenus (or Timoxenus) *On Harbours*, and he is likely to have been concerned with Salamis harbour, not with the island of Salamis. The ancient harbour of Salamis town was probably on both sides of the modern Cape Kamateró, that is on the north side of the neck of the promontory as well as in Ambeláki Bay. Finally, the beach *Sileniae* and the Cape Tropaea of the town Salamis can hardly be equated with the long rocky spit of Cape Varvári, which extends 4 miles from the town and has no strand or beach along its length. I conclude, then, that Cape Kamateró is Cape Tropaea, *Sileniae* is the beach on the northern side of Cape Kamateró; and Cape Varvári, as we have seen above, is Cape Cynosoura. It follows that the battle was fought in the waters north-west of Cape Kamateró.

On *Persae* 447, *νῆσός τις ἔστι πρόσθε Σαλαμίνος τόπων*, Positano f 96 v publishes the scholium, *τὴν Ψυττάλειαν φησιν ἥτις διέχει Σαλαμίνος σταδίοις πέντε· ἐν ταύτῃ φυγόντες οἱ ἡγεμόνες τῶν Περσῶν ὑπ' Ἀθηναίων ἀπώλοντο*. The figure 'five' in this new scholium confirms the reading 'five' which occurs four times in the earlier scholia published by Dähnhardt in contrast to the two variants 'one hundred and five'. As Judeich, p. 129, observed, this precise measurement probably came also from Philoxenus (or Timoxenus) *On Harbours*. In that case, if the tip of Cape Kamateró, i.e., Cape Tropaea, marks the entry of the Salamis harbour, we should expect the distance from there to *Ἅγιος Yeóryios*, i.e., Psyttalia, to be five stades (1,000 yards). It is in fact almost exactly 1,000 yards if we measure to the modern jetty on *Ἅγιος Yeóryios*, a jetty which may have existed in the fourth century B.C. (and later) when Psyttalia was named 'the eyesore of the Peiraeus' (*Strabo* C 395; cf. p. 36 above). By a perverse chance the distance from the tip of Cape Varvári to Lipsokoutáli is also about 1,000 yards; those who wish to identify Psyttalia with Lipsokoutáli can bring this measurement to bear, if they are prepared to argue that *Σαλαμίνος* in the Scholium means the island Salamis.

Two arguments which have been based on the Scholia may be mentioned only to be dismissed. One is that the ancient beach *Sileniae* should be equated with the modern beach *Selinia*; but this will not do, because the wind in the closing phase of the battle was westerly and not easterly. The other is the proposal of Beloch to restore the words *πρὸς ζέφυρον* in the defective text of Scholia Codicis M, *τὴν Ψυττάλειαν φησιν ἥ ἀπέχει πρὸς . . . ρον* (inter *πρὸς* et *ρον* *trium fere litterarum spatium posito* *στ* in marg.m.) *σταδίους ἑ, ὅπου φυγόντες οἱ ἡγεμόνες τῶν Περσῶν ὑπ' Ἀθηναίων ἀπώλοντο*. But his proposal is no more probable than Judeich's proposal to restore *πρὸς εὖρον* or Dindorf's to restore *πρὸς ἡπειρον*.



## ZEUS IN AESCHYLUS<sup>1</sup>

AFTER a hundred and thirty years of controversy, the interpretation of the *Prometheus Bound* is still the subject of debate. To the romantic poets of the revolutionary era, the Titan tortured by Zeus for his services to mankind appeared as a symbol of the human spirit in its struggle to throw off the chains which priests and kings had forged for it.<sup>2</sup> But to the distinguished Hellenists who after the fall of Napoleon laid the foundations of the great century of German scholarship, no such naïve and one-sided view of the *Prometheus* seemed tolerable. It was partly, perhaps, that the political atmosphere discouraged an interpretation adverse to authority; but the other writings of Aeschylus himself seemed to offer strong evidence against this view. Elsewhere in Aeschylus, they could argue, Zeus was treated with profound respect. In the *Suppliants*, he is continually appealed to by the chorus of Danaids, who miss no opportunity to extol the supremacy of his power. Zeus' omnipotence is the burden of a celebrated section of the first chorus of the *Agamemnon*; although scholars have differed widely in the details of their interpretation of this passage, most are agreed that it expresses theological doctrines that are at once subtle and sublime, and many have discovered profound significance in its puzzling allusions to 'learning by suffering' and to a χάρις that comes to men from the gods. Chiefly upon evidence derived from these two plays, many scholars have credited their author with the invention of a peculiar personal religion, tending to exalt Zeus at the expense of the other members of the Olympic pantheon, and crediting him with the purpose of perfecting men in goodness through the discipline of suffering. Some have gone so far as to detect tendencies to monotheism in Aeschylus.<sup>3</sup> A fair specimen of the usual kind of view is that of Nilsson,<sup>4</sup> who begins by hesitating to pronounce Aeschylus a monotheist; Aeschylus, he warns us, is not a religious innovator preaching a new form of religion, but a profoundly pious poet;<sup>5</sup> but later, he comes dangerously near to this view. The power of Zeus, he argues, is so much magnified that at one point he seems more a principle than a personal god.<sup>6</sup> In dealing with the famous fragment of the *Heliades* (fr. 70):

Ζεύς ἐστὶν αἰθὴρ, Ζεὺς δὲ γῆ, Ζεὺς δ' οὐρανός,  
Ζεὺς τοὶ τὰ πάντα χῶτι τῶνδ' ὑπέρτερον.

Nilsson leaves open the possibility that the poet 'went yet further', and embraced a pantheistic view of nature.<sup>7</sup> Twice he speaks of 'Aeschylus' Zeus-religion', and finally he remarks with some surprise that it had no influence on posterity.<sup>8</sup>

Here, then, is the problem which perplexed the contemporaries of Hermann and Welcker and which still perplexes us: how is the presentation of Zeus given in the *Prometheus Vinculus* to be reconciled with the picture of him found in the other plays of Aeschylus?

This disagreement is in large measure due to our ignorance regarding the trilogy of which the surviving *Prometheus* play formed part. Of the Προμηθεὺς Λυόμενος we possess a number of fragments, and it is clear that the release of Prometheus by Heracles, predicted in the existing play, occurred in this work, as its name implies. Of the Προμηθεὺς Πυρφόρος, on the other hand, we know scarcely anything. Welcker's opinion that it was the first play of the trilogy<sup>9</sup> has been generally given up since Westphal pointed out that the narrative of past events in the *Prometheus Vinculus* makes it improbable that another play had gone before.<sup>10</sup> But the extreme rarity of quotations lends colour to the suspicion that Προμηθεὺς Πυρφόρος may be simply another way of referring to the satyr-play

<sup>1</sup> This paper was read to the Oxford branch of the Classical Association in November 1953. I gratefully acknowledge the kind help of Professor D. S. Robertson, Professor E. R. Dodds, Mr. G. S. Kirk, and in particular Professor D. L. Page.

<sup>2</sup> See K. Heinemann, *Die Tragischen Gestalten der Griechen in der Weltliteratur*, pp. 12 f.; O. Walzel, *Das Prometheus-Symbol von Shaftesbury zu Goethe*; L. Séchan, *Le Mythe de Prométhée*, pp. 15 f., 95 f.

<sup>3</sup> First Haas in *Archiv für Religionswissenschaft*, iii (1900), pp. 163 f.; see the list of writers taking this view given by Wm. Nestle, *Griechische Studien*, p. 66, n. 14. So also H. J. Rose, *A Handbook of Greek Literature*, p. 159: 'Practically, Aeschylus was a monotheist.'

<sup>4</sup> *Griechische Religionsgeschichte* I, pp. 707 f. The list of books and articles whose authors hold this kind of view about religion in Aeschylus could be made very long indeed. See, for example, Schmid-Stählin, *Griechische Literaturgeschichte* I ii, pp. 265 f.; Wilamowitz, *Der Glaube der Hellenen* II, pp. 133 f.; M. Pohlenz, *Die Griechische Tragödie* I, pp. 141 f.; Wm. Nestle, *Neues Jahrbuch*, 1907, pp. 225-46, 305-33 = *Griechische Studien*, pp. 61 f.; G. Murray, *Aeschylus. The Creator of Tragedy*; W.

Jaeger, *Paideia* I, pp. 263-4 (English version); W. K. C. Guthrie, *The Greeks and their Gods*, pp. 65, 258; F. M. Cornford, *Plato's Cosmology*, pp. 361 f.

<sup>5</sup> P. 708, top: 'er ist kein Systembauer, sondern ein von einem hohen Glauben an seine Religion ergriffener Dichter'.

<sup>6</sup> P. 711, near top: 'seine Macht wird so gesteigert, dass er an einer Stelle mehr als ein Prinzip denn als ein persönlicher Gott erscheint'. The 'one place' is, of course, Ag. 160 f., on which see below.

<sup>7</sup> Other evidence for pantheism in Aeschylus is lacking: and without a knowledge of the context of this fragment it is unsafe to argue from it. In default of any evidence to support the pantheistic interpretation, I suspect that it means much the same as the concluding line of the *Trachiniae*:

κούδεν τούτων δὲ τι μὴ Ζεύς.

<sup>8</sup> P. 711: 'auf die Religion der Nachwelt hat seine Zeus-religion kaum Einfluss gehabt'.

<sup>9</sup> *Die Aeschyleische Trilogie Prometheus*, 1824, pp. 7 f.

<sup>10</sup> *Prolegomena zu Aeschylus' Tragödien*, 1869, pp. 207 f.



produced with the *Persae*, *Glaucus Potnious* and *Phineus* in 472 B.C. and called, in all probability, the Προμηθεὺς Πυρκαεὺς. Professor D. S. Robertson has suggested<sup>11</sup> that the author may have died without putting the finishing touches to the trilogy and that some of the odes may have been supplied by one of his sons, Euphorion and Euaion, or by his nephew, Philocles. Without new evidence this theory cannot be proved right or wrong; but the convenient explanation which it offers of the abnormalities of style and language<sup>12</sup> and the yet more striking abnormalities of metre<sup>13</sup> presented by the *P.V.* make it a possibility which must be taken seriously. In another place I hope to discuss in detail the limited evidence which bears on the reconstruction of the trilogy; here, I wish to concentrate upon the central problem of the conflict between Zeus and Prometheus.

Some scholars have contended, against all appearances, that the sympathy of the audience is meant to lie, not with Prometheus, but with Zeus. Others have argued that the remaining play or plays of the trilogy must have shown a development in Zeus' character; from the savage tyrant of the *P.V.* must have emerged the beneficent monarch of the *Supplices* and *Agamemnon*. Others, unwilling to accept either view, have denied that the surviving Prometheus play can be by Aeschylus, a view to which the abnormalities referred to certainly lend some colour.<sup>14</sup> The first of these three opinions was originated by Schoemann,<sup>15</sup> was taken over by Wecklein,<sup>16</sup> and is familiar to English readers from the school edition of Sikes and Willson (Macmillan, 1898), which after fifty years is still the most useful commentary to the play in our language. Prometheus, Schoemann argued, did mankind no real good; all he did was to invent science and technology, mixed blessings. Holders of this view describe the conflict in the language of Christian or Jewish theology; Prometheus has sinned and is receiving just punishment, and in the last play he will repent and acknowledge his transgression. Some of them even argue that the conduct of Zeus described in the *P.V.* was only part of a higher design for the destiny of mankind,<sup>17</sup> a design oddly reflected in Zeus' intention, frustrated only by Prometheus, of exterminating them (see l. 231-4). This opinion has gone out of fashion during the present century, and no wonder.<sup>18</sup>

Dissen<sup>19</sup> was the first to suggest that the trilogy showed an evolution in the character of Zeus, an evolution which by the beginning of the last play of the trilogy must have transformed the youthful tyrant of the *Prometheus Vincit* into a beneficent ruler, who of his own free will releases his noble enemy. This is a view which during the last century remained comparatively unpopular, but which during the present one has become accepted by most scholars. It was adopted by Wilamowitz;<sup>20</sup> and among living scholars it is held by Nilsson (*loc. cit.*), Festugière,<sup>21</sup> and Professor E. R. Dodds (in an unpublished paper read last year to the Leeds branch of the Classical Association, which he has with the greatest kindness allowed me to make use of). Wilamowitz (*Aischylos. Interpretationen*, p. 150) quoted in illustration of it the Semitic scholar Vischer's explanation of the savage cruelties perpetrated in the name of the Jehovah of the Old Testament: 'In those days the good God himself was young'; and he drew attention to the constant stressing, in the *P.V.*, of the youth of Zeus and the newness of his power. By way of parallel, the holders of this opinion point to the change of character they say is experienced by the Eumenides in the play named after them.

Is the notion of a god thus changing in character one that Aeschylus can possibly have entertained? Schmid (*op. cit.*, p. 89) flatly asserted that it was impossible; the Greek notion of divine growth, he contended, is expressed in Callimachus' address to Zeus:

ἀλλ' ἔτι παιδὸς ἔων ἐφράσσαο πάντα τέλεια (Hy. 1, 57).

And since Schmid wrote, two eminent authorities on Greek religion, Farnell and Karl Reinhardt,<sup>22</sup> have expressed agreement.

<sup>11</sup> *Proceedings of the Cambridge Philological Society*, 1938, pp. 9-10.

<sup>12</sup> See especially J. Wackernagel, *Verhandlungen der 46. Versammlung der Philologen*, 1902, pp. 65 f.; *Studien zum Griechischen Perfektum*, pp. 11 f.; J. D. Denniston, *The Greek Particles*, pp. lxxvii f.; see bibliography in W. Schmid, *Untersuchungen über den Gefesselten Prometheus*, pp. 1-4 and cf. pp. 41 ff. of that work.

<sup>13</sup> See E. C. Yorke, *CQ* 30 (1936), pp. 116 f., pp. 153 f.; J. D. Denniston, *ib.*, pp. 73 f., 192 f.; W. Kranz, *Stasimon* 226-8.

<sup>14</sup> Wackernagel seems to have arrived at this opinion for purely linguistic reasons: see n. 12 above.

<sup>15</sup> *Opuscula* iii 81 f., 95 f., 120 f.; see also the introduction to his edition of the play.

<sup>16</sup> In the introduction of his edition of the play (2nd. edn., 1878; English translation by F. D. Allen, Boston, 1891).

<sup>17</sup> See J. A. K. Thomson in *Harvard Studies in Classical Philology* 37, 1920.

<sup>18</sup> It has been revived in an extreme form by E. Vandvik (*Norske Videnskaps Akademi, Hist.-Filos. Klasse*, 1942, 2); the experiment does not seem to me successful.

<sup>19</sup> *Ap. Welcker, op. cit.*, pp. 40 f.

<sup>20</sup> *Aischylos, Interpretationen* (1914), pp. 149-50; *Der Glaube der Hellenen*, II 133-4.

<sup>21</sup> *Revue de Philologie* 22, 1948, pp. 156 f. Cf. E. Myers in *Hellenica* (ed. E. Abbott), 1880, p. 20; L. Campbell, edn. of *Prometheus Vincit*, 1890, introd.; Janet Case, *CR* 1904, pp. 99 ff.; H. Weil, *Études sur le Drame antique*, pp. 82 f.; S. H. Butcher, *Harvard Lectures on Greek Subjects*, pp. 17 ff.; Wm. Nestle, *loc. cit.*; J. T. Sheppard, *Greek Tragedy*, pp. 62 f.; P. Mazon, *Eschyle I*, pp. 153 f.; M. Croiset, *Eschyle*, p. 163; J. A. K. Thomson, *loc. cit.*, p. 34; O. J. Todd, *CQ* 1925, pp. 61 ff.; M. Pohlenz, *Die Griechische Tragödie I*, pp. 76 ff.; G. Thomson edn. of *Prometheus Bound*, pp. 12 f.; A. Bonnard, *Rev. de Theol. et de Phil.*, 1933, pp. 206 f.; F. Vian, *Rev. des Et. Gr.*, 1942, pp. 190 f.; F. Solmsen, *Hesiod and Aeschylus*, pp. 147 f.; H. D. F. Kitto, *JHS* 1934, pp. 14 f.; *id. Greek Tragedy*, p. 64, n. 1; L. Séchan, *Le Mythe de Prométhée*, pp. 55 f.

<sup>22</sup> *Aischylos als Regisseur und Theologe* (Basel, 1951), pp. 68 f. Reinhardt makes the important remark that the stress laid upon the youth of Zeus in the *P.V.* may well be explained by the desire to contrast the older gods, to whom Prometheus belonged, with the younger generation of Zeus and his brothers and sisters. For a similar contrast, cf. *Eum.* 150, 731, 778-9, 808-9, 882-3.



Believers in the development theory have certainly failed to support it by any convincing parallel. That of the Eumenides was exploded by Farnell, who points out (*loc. cit.*, p. 47) that they do not change in character. A handsome offer from Athene, backed by the threat of Zeus' thunder (*Eum.* 826-8), induces them to abandon their design of avenging upon Athens the acquittal of Orestes, and to take up their quarters on the Acropolis. They do not repent of their earlier persecution of Orestes, any more than they hand over their duty of pursuing the slayers of their own kin to the court of the Areopagus or to the Athenian state, as so many scholars have implied they do. And without a parallel, the development theory is not easy to defend. The most ingenious argument for it known to me is that of Professor Dodds, who argues that its opponents have confused the character of the Aeschylean Zeus with that of the eternal and unchanging god whose first appearance in Greek thought is no earlier than Plato and Aristotle. The Zeus of the fifth-century poets, he points out, differed from this god in being within time. Zeus had not always been the monarch of the universe, but Ouranos and Kronos had gone before him; Zeus had been a child, and his birth-place could be visited.<sup>23</sup> No one can deny that the Zeus of Aeschylus' age was within time; but one may question whether it follows from this that his character could develop. Professor Dodds draws attention to the constant stressing of the youth of Zeus, and argues that this may indicate that when Zeus was older he will have become wiser.<sup>24</sup> But Reinhardt has pointed out (*loc. cit.*, p. 68) that this can easily be explained by the poet's desire to contrast the older generation of gods, to which Prometheus belonged, with the younger generation of Zeus and his brothers; for a similar contrast between older and younger gods, see *Eum.* 160, 731, 778-9, 808-9, 882-3. The advocates of the development theory fail to support it by adducing any certain example of such a change of character in any god. And I shall argue later (see p. 66) that, in view of what we are told about Prometheus' knowledge of the danger that threatens Zeus, the development theory is not necessary to the working out of the plot.

I have given reasons for hesitating to accept either the view that Zeus throughout the trilogy must have been seen in a more sympathetic light than that in which the surviving play reveals him or the view that the later play or plays showed a development in Zeus' character. Schmid, who rejected both these opinions, felt himself obliged to deny the authenticity of the single surviving play;<sup>25</sup> the linguistic and metrical evidence which he adduced, while by no means lightly to be brushed aside, would not, I suspect, have led him to so extreme a step had not the content of the play conflicted with his whole conception of Aeschylus. He held that the presentation of Zeus in the other plays of Aeschylus was one so elevated, so advanced, and so profound that its author could not possibly have written the *Prometheus Vincitus*, with its very different picture. No, this play must be by someone else; and Schmid invented a sophist of the last quarter of the fifth century, probably an Ionian and certainly a blasphemer and a degenerate, to serve as whipping boy.

Now if the development theory is regarded with suspicion, as I think it must be, how can Schmid's view or one like it be ruled out? Can this be done by questioning any of the assumptions common, so far, to all investigators of the problem? All alike have maintained the purity, nobility, and refinement of the theology of the other plays of Aeschylus, so utterly at variance with the theology of the *Prometheus Vincitus*. It may be worth while to re-examine the evidence on which this assumption rests.

I will begin with the *Supplices*, which has more about Zeus than any other play of Aeschylus but the *P.V.*, and which has supplied much of the evidence alleged in favour of an Aeschylean Zeus-religion. Apart from occasional invocations of Zeus as ξένιος (627, 672 cf. *Ag.* 61, 362, 748), σωτήρ τρίτος (26 cf. *Eum.* 759-60, fr. 55, 4) or ἀλεξητήριος (8), the chorus of Danaids appeals to him in three capacities; as their ancestor through Epaphos, his son by Io, as Hikesios, the protector of suppliants, and as the special defender of the rights of Dike, Justice. There are also a number of invocations which lay special stress on the supremacy of Zeus' power. The first two of these four aspects of Zeus do not here require investigation. Any other Olympian might have been the divine ancestor of the chorus; and any other Olympian might have had the protection of suppliants as a special charge. The two aspects that remain must be examined; I will take first the last-mentioned. The power of Zeus is celebrated above all in two well-known passages, one in the parodes (86-103) and the other at the end of the second stasimon (590-99).

<sup>23</sup> See Wilamowitz, *Der Glaube der Hellenen* I, pp. 127-32.

<sup>24</sup> Professor Dodds supports his argument by a reference to Moschion fr. 6 (Nauck<sup>2</sup>, pp. 813-14). This fragment describes the early time when men lived like beasts without houses, crops, or tools:

ἐν δ' οὐ νόμος  
ταπεινός, ἢ βία δὲ σύντροφος Διί.

At last Time, the mother and nurse of all things, educated them:

εἴτ' οὖν μέριμναν τὴν Προμηθεὺς πάρα  
εἴτ' οὖν ἀνάγκην εἴτε τῇ μακρᾷ τριβῇ  
αὐτὴν παρασχὼν τὴν φύσιν διδάσκαλον.

and they tilled the soil, built cities, and buried their dead. Professor Dodds suggests that Moschion wrote with the Prometheus-trilogy in mind; he thinks the first of the two passages I have quoted may allude to the replacement, at the end of the trilogy, of Bia by Dike as the πάρεδρος of Zeus. Now in line 16 it is not certain that Διί is the right reading; both the MSS. of Stobaeus originally read νηί, which has been corrected to Διί in P. The correction may be right, but Canter's δίκη has slightly more palaeographical probability. But even if Διί were unquestionably right, the inference regarding the Prometheus-trilogy would not be certain.

<sup>25</sup> *Op. cit.* With ludicrous over-confidence, he treats this conjecture of his own, in his history of Greek literature, as an established fact: Schmid-Stählin, *op. cit.*, I ii 193, 281 f.



- †εἶθ' εἴη Διὸς εὖ παναληθῶς  
 Διὸς ἡμερος.† οὐκ εὐθήρατος ἐτύχθη;  
 παντᾶ τοι φλεγέθει  
 κἂν σκότῳ μελαίνα σὺν τύχᾳ  
 90 μερόπεςσι λαοῖς.  
 πίπτει δ' ἀσφαλὲς οὐδ' ἐπὶ νώτῳ  
 κορυφᾷ Διὸς εἰ κρανθῇ πρᾶγμα τέλειον  
 δαυλοὶ γὰρ πραπίδων  
 δάσκιοί τε τείνουσιν πόροι  
 95 κατιδεῖν ἄφραστοι.  
 ἰάπτει δ' ἐλπιδων  
 ἀφ' ὑφιπύργων πανώλεις  
 βροτούς, βίαν δ' οὐτιν' ἐξοπλίζει.  
 100 πᾶν ἄπονον δαιμονίων.  
 ἡμενος ὃν φρόνημά πως  
 αὐτόθεν ἐξέπραξεν ἔμ-  
 πας ἐδράνων ἀφ' ἄγνων

100 *em.* Wellauer: τὰν ἄπονον δαιμονίων M. 101 *em.* Paley: ἡμενον ἄν (= ἄνω) M.

The opening of the first is probably corrupt, and has not been convincingly emended; 'would that the desire of Zeus may in all truth be of Zeus' is very curious sense. The chorus continues: 'Is not it (he?) easy to track down? See, everywhere it (he?) blazes, even in the darkness, with a black fate for mortal peoples. And it falls safely and not on its back, any act that the nod of Zeus ordains shall be accomplished. For thick and bushy do the paths of his mind extend, inscrutable to the eye. And from their high towers of hope he hurls mortals to utter destruction, yet he arms no violence against them. Effortless are all things for the gods: seated he somehow accomplishes his purpose, come what may, from his sacred seat.' The passage from the second stasimon runs as follows:

- 590 τίν' ἂν θεῶν ἐνδικωτέροιςιν  
 κεκλοίμαν εὐλόγως ἐπ' ἔργοις;  
 αὐτός ὁ πατήρ φυτουργὸς αὐτόχειρ ἄναξ,  
 γένους παλαιόφρων μέγας  
 τέκτων, τὸ πᾶν  
 μῆχαρ, οὐριος Ζεὺς.  
 595 ὑπ' ἀρχᾶς δ' οὐτινος θαόζων  
 τὸ μείον κρεῖσσον ὦν κρατύνει.  
 οὐτινος ἄνωθεν ἡμένου σέβει κάτω.  
 πάρεστι δ' ἔργον ὥς ἔπος.  
 σπεῦσαί τι τῶν  
 βούλιος φέρει φρήν.

'Which among the gods might I invoke with reason on the strength of juster actions? The Father himself is the King whose very touch begat us, the mighty architect of our race of old is he who can cure all ills, the kindly Zeus. And seated beneath no other's power, he that is greater rules over what is less. He does not pay reverence below while another sits above; his is the power to accomplish a deed as easily as a word, any deed his counselling mind brings forth.'

It is important to recognise that this exaltation of Zeus' power is not new. These passages recall the nod with which Zeus assures Thetis of his help at *Il.* I 524-30 and Zeus' boasting that he could draw up gods and earth and sea upon a golden chain at *Il.* VIII 18-27. Zeus in Homer may seem a less remote and dignified figure than Zeus in Aeschylus outside the *P.V.* But this may be simply because he is shown at closer range. In the *Iliad* just as much as in Aeschylus, Zeus effectively controls the action: Διὸς δ' ἐτελείετο βουλή. This may apply to the Zeus of the *P.V.* also.

The Aeschylean Zeus, like the Homeric, has power to accomplish any action, or almost any action, by his mere fiat; he is, in one sense, omnipotent, and is called so three times in Aeschylus (*Sept.* 255, *Supp.* 816, *Eum.* 918; cf. *Supp.* 1048-9, *Eum.* 650-1, *P.V.* 61). But at this point Professor Dodds's warning against confusion between the fifth-century Zeus and the later θεὸς αἰώνιος of the philosophers must be born in mind, though the context is different from that in which it was originally delivered. Zeus' omnipotence is limited, and so is different from the kind of omnipotence that is familiar to modern thinking. Some of its limitations are ascribed to an unwillingness to transgress the decrees of fate; as in the *Iliad* Zeus regretfully decides not to save Sarpedon, so in the *Agamemnon* (1022-6) and the *Eumenides* (644-51) he is said to have forbidden others to raise men from the dead, and we infer that he would not do this himself. One limitation of his power does not seem voluntary; in the *P.V.*, he cannot kill the immortal Prometheus (1053; cf. 933). Moreover Zeus is



not safe against the remoter consequences of his own actions; just as Kronos begat a son more powerful than himself, so will Zeus also, unless Prometheus warns him first. The will of the later philosophic god is identical with the will of destiny; that of the Aeschylean Zeus is not. In the *P.V.* (518), we learn that Zeus cannot escape Fate; and the last words of the *Eumenides* imply a similar distinction between their powers.

Just as Zeus is omnipotent, but not omnipotent in the modern sense, so is he eternal, but not eternal as we use the word. The rule of Zeus, says the chorus of the *Suppliants*, will never end (574, δι' αἰῶνος κρέων ἀπαύστου); but unlike that of the Platonic god, it had a beginning. Another epithet of the philosophic god that has been misleadingly applied to Zeus is 'perfect'. At the opening of the second stasimon of the *Suppliants* (524), Zeus is invoked as follows:

ἀναξ ἀνάκτων, μακάρων  
μακάρτατε καὶ τελέων  
τελειότατον κράτος, ὄλβιε Ζεῦ.

'King of Kings, most blessed of the blessed ones, strength mightiest to accomplish of all that have this power.' Professor Fraenkel's note on *Ag.* 973 well brings out the sense of the epithet τέλειος. The common rendering 'most perfect of the perfect' is as inaccurate as it is vague, and carries with it a most misleading suggestion of metaphysical perfection.

I come now to the conception of Zeus as the special champion of the rights of Dike. At *Supp.* 143 f., the chorus appeals to Dike as the daughter of Zeus; cf. *Cho.* 948 f., where her name is probably etymologised as Διὸς κόρα. Zeus holds the balance and dispenses injustice to the wicked and holiness to the law-abiding (403 f.); the power of Zeus is just (437); Zeus is implored to look with hostile eye, 'in accordance with justice', upon the violence of the pursuers (811 f.). This connection also receives special stress in the *Seven Against Thebes*; there we are repeatedly told that Zeus and Dike are on the side of Eteocles and the defenders; this is implied at 443-6, 565-7, and 630, and is clearly stated at 662-71, where Eteocles calls Dike the maiden daughter of Zeus and affirms that Polyneices, from his earliest years, has had no part in her. There is more about the relation of Zeus and Dike in a fragment of great interest published in Volume XX of the Oxyrhynchus Papyri (P. Oxy. 2256, fr. 9(A)), of which I reproduce the text with Mr. Lobel's supplements. The fragment has not been assigned to any particular play,<sup>26</sup> but is probably from a satyr-play. The speaker of the ῥῆσις with which the fragment begins is clearly Dike herself; she is explaining the manner in which she came by her historic privileges.

μακάρων . [   
 αὐτηθεῶν[   
 .] α . . . λ<sup>λ</sup> . . . πε<sup>π</sup>ε . δ . [   
 .] . [ . . . . . ] . ν . [   
 5 ἰζει δ' ἐν αὐτῷ . [ . ] . . [ . ] . . [ . ] . [   
 δίκη κρατήσας τῷδε . [   
 πατήρ γάρ ἦρξεν, ἄνταμ[   
 ἐκ τοῦ δέ τοί με Ζεὺς ἐτίμ[ησεν   
 ὀτιή παθῶν ἡμ[ . ] . [   
 10 ἰζω Διὸς θρόνοισιν[ . . ] ισμένη[   
 πέμπει δέ μ' αὐτὸς οἰσιν εὐμεν[   
 Ζ[ε]ὺς, ὅσπερ ἐς γῆν τήνδ' ἐπεμψέ μ' . . [   
 . [ . ] εἴθε δ' ὑμεῖς εἴ τι μὴ μά[την] λέγω .   
 — . [ . ] οὐ[ν] προ[σεννέποντες] εὐ . [ . ] ἡσόμε[ν] ;   
 15 — Δίκην μι[ . . ] ον πρεσβο . η . ε . . . ρο . [   
 — ποιᾶς δέ τ[ιμ]ῆς ἀρχ . . . . . εἰσα[   
 — το]ῖς μὲν δ[ι]καίοις ἐνδίκον τειν . . ο[   
 — . ] σα θέ[σ]μ[ι]ον τὸδ' ἐν βρ[ο]το[ῖς]   
 — τοῖς δ' αὖ μα[ταίοις] . [ . ] . [ . ] . . . [ . ] . . . φ . [   
 20 — πότερον ἐ[πι]πιδαις ἡ κατ' ἰσχύος τρόπο[ν] ;

<sup>26</sup> Professor D. S. Robertson has suggested (*CR* 1953, pp. 79-80) that the person whose mischievous behaviour is described in the concluding lines is Ares, and that Dike is leading up to an account of his trial for the murder of Halirrhothius. Ares, however, is nowhere else credited with such a persecution of travellers as that described in lines 34 f.; and one recalls that this feature would suit Cynus, not the son of Poseidon killed by Achilles, but the son of Ares killed by Heracles. This Cynus used to harry pilgrims on the way to Delphi (*Hes. Sc.* 478-80, *Eur. H.F.* 389-94).

Wecklein and Nauck credit Aeschylus with a play *Κύνος*, but only tentatively, because of *Ar. Ran.* 963:

Κύνους ποιῶν καὶ Μέμνονας κωδωνοφαλαρακόμους.

Professor Robertson has warned me that in this passage the association with Memnon suggests that the other Cynus is meant (cf. *Pindar Ol.* ii, 82 and *Isth.* v, 39). Still, the Hesiodic Cynus is a familiar figure in literature and art (cf. *Hes. Sc. passim*; *Pind. Ol.* x, 15; *Eur. H.F.*, *loc. cit.*; *Alc.* 503; see Engelmann in *Roscher's lexicon*, p. 1690; he occurs ten times in Dr. Jacobsthal's mythological index to Sir John Beazley's *Attic Red-Figure Vasepainters*, the other Cynus never). But it is hard to think of a supplement that will accord with this suggestion. For another suggestion about this fragment, see *Ed. Fraenkel in Eranos* lii, 1954, 61 f.







Fishes, beasts, and birds, we are told (274 ff.), eat one another, as there is no Dike among them. To men, on the other hand, Zeus has given Dike, which is far the best of all things; for to the man who recognises what is just and is willing to speak it out, Zeus gives prosperity; but as for the man who bears false witness upon oath and does outrage against Dike, his race after him becomes feeble.

τόνδε γὰρ ἀνθρώποισι νόμον διέταξε Κρονίων.  
 ἰχθύσι μὲν καὶ θηρσί καὶ οἰωνοῖς πετεηνοῖς  
 ἐσθήμεν ἀλλήλους, ἐπεὶ οὐ δίκη ἐστὶ μετ' αὐτοῖς.  
 ἀνθρώποισι δ' ἔδωκε δίκην, ἣ πολλὸν ἀρίστη  
 γίγνεται. εἰ γὰρ τίς κ' ἐθέλη τὰ δίκαι' ἀγορεύσαι  
 γινώσκων, τῷ μὲν τ' ὄλβον διδοὶ εὐρύοπα Ζεὺς.  
 ὃς δέ κε μαρτυρήσῃ ἔκων ἐπιόρκον ὁμόσσης  
 ψεύσεται, ἐν δὲ δίκην βλάψας νήκεστον ἀάσθη,  
 τοῦ δὲ τ' ἁμαυροτέρῃ γενεῇ μετόπισθε λέλειπται.  
 ἀνδρὸς δ' εὐόρκου γενεῇ μετόπισθεν ἀμείνων.

With this doctrine the Oxyrhynchus fragment, and also the *Supplices* and the *Septem*, is in no way inconsistent. It remains to enquire whether the *Oresteia* provides any evidence for an exalted and sophisticated conception of Zeus.

The chorus of the *Agamemnon* in its opening anapaests declares that it is Zeus, Zeus Xenios, who has sent the Atreidae against Troy; the purpose of Zeus is being worked out, and no attempt to appease him can succeed (60 f.). Of this we are reminded at the beginning of the first choral ode. The portent of the eagles comes from Zeus, and it carries with it Zeus' promise that the expedition shall achieve its purpose. But the portent 'though favourable, is fraught with blame':

τούτων δ' αἰτεῖ ξύμβολα κραῖναι  
 δεξιὰ μὲν, κατάμομφα δὲ φάσματα (144-5)

The subject of the sentence is Artemis; and whom is Artemis asking to fulfil the portent? The common use of κραίνειν to describe Zeus' making valid of his ordinances, as well as the general sense of the passage, suggests that it is Zeus. To supply Δία as the subject of κραίνειν is certainly not easy. But in this context the likelihood of a reference to Zeus is considerable; and there is a passage of the *Prometheus* which seems to me to supply a parallel. At *PV.* 99-100 Prometheus says:

φεῦ φεῦ, τὸ παρὸν τό τ' ἐπερχόμενον  
 πῆμα στενάχω, ποῖ ποτε μόχθων  
 χρή τέρματα τῶνδ' ἐπιτεῖλαι.

Most editors think that τέρματα is the subject of the oratio obliqua clause here; and they take ἐπιτεῖλαι as a rare instance of the active of this verb being used, as the passive so often is, of the rising of stars or planets or of things that are compared to stars or planets. So they arrive at the translation 'when is an end of my labours fated to arise'. Another way of explaining this passage would be to take ἐπιτεῖλαι in its common sense of 'to command' and to supply Δία from the preceding sentence as its subject: then one would render it 'when is he destined to ordain the end of these labours?' Why then is ποῖ used where we should expect πότε? ποῖ occurs with a verb meaning 'to end' to denote the point in time at which something is to end at *Pers.* 735 etc.; see Index Aeschyleus s.v. ποῖ II; and here τέρματα . . ἐπιτεῖλαι has the force of such a verb. This seems to me open to rather less objection than the usual way of dealing with the problem.

The chorus, then, is faced with a dilemma; Zeus has sent the Atreidae against the Trojans, but Zeus will concede to Artemis her demand for vengeance against the tearers of the hare. True, Zeus' responsibility is most obscurely indicated; but it may fairly be argued that the reluctance of both Kalchas and the chorus to admit it openly supplies a good reason why it should be darkly suggested instead of clearly stated. In his warning against the dangers that will follow, Kalchas does not only prophesy the sacrifice of Iphigenia; he hints at still more grievous consequences of that sacrifice:

μῖμναι γὰρ φοβερά παλίνορτος  
 οἰκονόμος δολία μνάμων μῆνις τεκνόποιος (154-5)

The chorus' dilemma explains something which I have not seen otherwise accounted for: it explains why the chorus chooses this point to break off its narrative of the events at Aulis to appeal to Zeus. Zeus only has the power to stave off the danger of which the prophet has given warning. This is the context in which is set the famous passage which has so often been appealed to, seldom with any reminder of the nature of that context, by the believers in a 'Zeus-religion'.

'Whoever Zeus is, if this name is acceptable to him, by this do I address him.' This invocation is indeed singular; but its singularity has long since been explained in terms of a type of ritual



address not uncommon in ancient religion. In order to invoke a god successfully, you must address him by the right name; accordingly, you either enumerate a whole string of titles or, to save time and to ensure not leaving out the title which the god really does approve, you adopt the form we find here. Many scholars have acknowledged this, but have none the less argued that Aeschylus here makes use of a relic of primitive religion in order to serve advanced convictions of his own.<sup>29</sup> If any convincing evidence of an advanced or spiritual conception of Zeus can be adduced from Aeschylus, this possibility will receive confirmation; at this stage of the investigation, we can only observe that, taken by itself, the passage is wholly explicable as an echo of a primitive manner of invoking gods.

'Measuring all things against him, I can compare to him none but Zeus, if I am in truth to cast from my reflecting mind the vain burden.' What is 'the vain burden'? μάταιος and its cognates are often used of the 'vain' or 'frivolous' conduct of those who deny or defy the power of Zeus; and Professor Fraenkel (*op. cit.*, pp. 102-3) takes 'the vain burden' to mean the burden borne by those who think such thoughts. I cannot understand how this meaning can have been clear to the audience. The 'vain burden' is presumably a burden which the chorus is seeking to cast from its own mind. Perhaps this is simply the burden of worry about the fortunes of the house of Atreus. Perhaps it is the burden of trying to think of someone, other than Zeus, with whom Zeus can be compared, a burden which the chorus do, at this minute, jettison. Whatever it is, I doubt if this expression has any religious implication.

What follows shows little trace of an advanced conception; rather it recalls the crudest myths of Hesiodic cosmogony. Ouranos ruled first till Kronos overthrew him; then Kronos ruled till he, in turn, was overthrown by Zeus; sing to Zeus a hymn of victory and you will have managed to attain good sense.

'Good sense' in this connection means the recognition of the feebleness of men in comparison with the gods; in the next stanza it is echoed by φρονεῖν at l. 176 and σωφρονεῖν at l. 181 (see Fraenkel on l. 176 (p. 105)). What is the meaning of the first three lines of the next stanza, from which many scholars have extracted quasi-Tolstoyan theories of 'redemption through suffering'? Professor Fraenkel translates it: 'it is Zeus who has put men on the way to wisdom by establishing as a valid law "By suffering they shall win understanding"'. The use here of a familiar proverbial expression cannot be dissociated from a famous passage of the *Works and Days*, in which Hesiod solemnly warns his deplorable brother to choose Dike in preference to Hybris. In the end, he assures him, Dike will be triumphant:

δίκη δ' ὑπὲρ ὕβριος ἴσχει  
ἔς τέλος ἐξελοῦσα. παθὼν δέ τε νήπιος ἔγνω. (218-19)

Here we have the earliest application to the dealings of the gods with men of a proverbial expression which appears earlier in the Homeric phrase ῥεχθὲν δέ τε νήπιος ἔγνω (P32, Y198). How far has Aeschylus moralised or theologised this piece of popular wisdom? Many have used it to contend that he thought sufferings were sent by the gods to refine or purify the character of mortals. But the actual victims of this law in the *Oresteia*, Agamemnon, Clytemnestra, and Aegisthus, scarcely bear out this contention. They are not purified or ennobled; they are simply killed; and the after life to which they are consigned is not one that offers scope for moral evolution. Other people, it may be argued, will learn from their example. But it is not easy to see what those people will learn, if not the uselessness of defying Zeus' law; and this the victims themselves, in their last moments, may be said to learn also. Now though this doctrine is in a sense ethical, it cannot be called advanced ethics.

The stanza continues: 'And before the heart there drips, instead of sleep, pain that reminds of injury; and even to the unwilling discretion comes.' This clearly reinforces, in other words, what has been said immediately before. What comes next is very difficult. To brush aside the easy correction βίαιος as one dictated by subjective considerations is, I think, a mistake. The strongest argument for this change is a grammatical one; a perfect, as Professor Fraenkel has shown in his note on 407, describes a state and not an action, and therefore cannot be modified by an adverb of the sort that normally describes an action, such as βιάω. Not that this argument in favour of βίαιος is decisive. ἡμένων, unlike βέβοσκεν at 407, is a participle; and it is arguable that since the words σέλμα σεμνὸν ἡμένων are in effect equivalent to ἀρχόντων they can be modified by just such an adverb as ἀρχόντων might be.<sup>30</sup> In any case, the difference to the sense is not a great one. Keep βιάω and you get the sense, 'And it is, I think, a kindness coming from the gods who by violence sit on their dread bench.' Read βίαιος and you get: 'And it is, I think, a kindness done by violence that comes from the gods who sit on their dread bench.' Neither version leaves any excuse for sentimentalising this χάρις, which is clearly of the sort that would normally be described

<sup>29</sup> See Fraenkel *ad loc.* (*Agamemnon* II, p. 100).

<sup>30</sup> 'Mr. Conington observes with truth that βιάω means "violently" rather than "powerfully"; and on the strength of this he prefers βίαιος, and translates "strange as it may seem (now), the free gift of the gods is forced on men". But we

may fairly reply that the poet merely meant βιάω ἀρχόντων, "ruling by the law of constraint, and not allowing mortals to follow their own headstrong wills with impunity" (*Paley, ad loc.*).



as a χάρις ἀχαρίς. The word χάρις is being used ironically; what is described as a χάρις is the forcing upon men by the gods of the kind of σωφροεῖν that comes to them against their will. The notion that the maintenance by the gods of a definite scheme of punishment for offences against their laws lets us 'know where we are', and is therefore on that account called a χάρις seems to me far too hard to supply from the context, as well as far too sophisticated, for any audience to have understood it.<sup>31</sup>

To sum up, the so-called 'Zeus-hymn' seems to me to yield no evidence whatsoever in favour of 'advanced conceptions', let alone an 'Aeschylean Zeus-religion'. On the contrary, it is set entirely within the primitive framework of the theology of the *Works and Days* of Hesiod.

So is the part played by Zeus throughout the trilogy. At *Ag.* 1485 f., the Chorus finally confirm the fear already present in their minds during the parodos, and roundly affirm that Zeus is responsible for all that has passed:

ἰὼ ἡ διαὶ Διὸς  
παναιτίου πανεργέτα.  
τί γὰρ βροτοῖς ἀνευ Διὸς τελεῖται;  
τί τῶνδ' οὐ θεόκραντόν ἐστιν;

I do not think it possible to doubt that this is one of those passages in which the chorus pronounces a judgment which the poet wishes us to accept. One thinks at once of that passage of the *Niobe* which Plato singled out for special reprobation, the passage whose speaker asserts that the god first manufactures a pretext, when he is bent on the destruction of a mortal.<sup>32</sup> The surrounding context of these lines, which is known to us from a papyrus fragment published by Vitelli and Medea Norsa in 1933, makes it easier to recognise what should in any case have been surmised, that the speaker of these lines does not assert that Zeus punishes the innocent; a view which, if it could safely be regarded as the poet's own, would be at variance with a famous passage of the *Agamemnon* (750 ff.) in which the Chorus certainly seems to speak for Aeschylus in asserting the opposite opinion. But the *Niobe* passage says nothing about *why* Zeus decides utterly to destroy a house; it only says that, once he has decided to, he manufactures some immediate cause for trouble.

The Chorus of the *Agamemnon* recognises that Zeus has ordained the king's death; but it warns Clytemnestra that the law of Dike requires that she and Aegisthus in their turn shall pay the penalty; and in spite of her talk of a compact with the daimon, she knows that they speak the truth (1560-77). In the next play, Orestes is the instrument of this law; Apollo is not lying when he tells the Erinyes (*Eum.* 616 ff.) that every utterance of his shrine has behind it the authority of Zeus (cf. 17-19, 713, 797). The speech in the *Choephori* (264 ff.) in which Orestes implores Zeus to help him to his vengeance is one of great interest, though it has never been a favourite with the advocates of Zeus-religion. 'If you destroy the nestlings of the eagle (Electra and myself), how shall you have from such a hand as ours, the banquets that are your due? If you cut off the race of the eagle, you will never more be able to convey to mortals the signals that they must obey; and if this whole royal stock wither away, never more will it do duty to your altars on the days of the sacrifice of oxen.'

Note especially l. 255 ff.:

καὶ τοῦ θυτῆρος καὶ σε τιμῶντος μέγα  
πατρός νεοσσούς τούσδ' ἀποφθείρας, πόθεν  
ἔξεις ὁμοίας χειρὸς εὐθoinον γέρας;  
οὐτ' αἰετοῦ γένεθλ' ἀποφθείρας, πάλιν  
πέμπειν ἔχοις ἂν σήματ' εὐπειθῇ βροτοῖς.  
οὐτ' ἀρχικός σοι πᾶς ὁδ' αὐάνθεις πυθμὴν  
βωμοῖς ἀρήξει βουθύτοις ἐν ἡμασιν.

Neither here nor anywhere in the great Kommos which follows this scene does the alleged advanced conception of Zeus make its appearance; we find only an appeal (382 f.) to Zeus as the sender of ὑστερόποινον ἄταν to the evil among mortals. The later utterances of the Chorus conform to the pattern we have come to expect. The sword of Dike will strike down those who have transgressed against the majesty of Zeus (639-45); Zeus is prayed to grant Orestes victory at 783 ff.; after the killings, the Chorus declares that Dike is their author (947 ff.).

<sup>31</sup> W. Headlam (*Agamemnon*, p. 187, followed by F. M. Cornford, *Plato's Cosmology*, p. 361) suggests that ποῦ might be written instead of ποῦ, and the sentence taken as a question; he would then translate, 'Where is there any joy of deities who sit upon their awful seat violently?' He may be right in taking the sentence as a question, but his translation scarcely suits the sense. If the sentence were a question, it would be better rendered, 'And where is the favour shown by the gods who sit on their dread bench through violence?' A close parallel is found in the desperate appeal of Croesus on the funeral pyre

(*Bacchyl.* 3, 37-8): ὑπέρβιε δαίμον, [ποῦ] θεῶν ἐστὶν χάρις; where χάρις means 'grace', 'favour', 'gratitude'. In this context, the Chorus may well ask where is the χάρις of the gods; the implication would, of course, be that it is hard to see any. Cf. P. Oxy. 2251, 4-5 (fr. 280 in my appendix to the Loeb edition).

<sup>32</sup> H. J. Mette, *Supplementum Aeschyleum*, fr. 116, l. 14-15; D. L. Page, *Greek Literary Papyri* I, p. 8. Loeb, *Aeschylus* fr. 277.



One of the many advantages to be got by ceasing to demand from Aeschylus a sophisticated and advanced theology is that of being free to recognise the naïve dramaturgy of the *Eumenides* for what it is. The issue lies between the law of Zeus, who as the champion of Dike demands through his προφήτης Apollo that the doer shall suffer, and the ancient τιμή of the Erinyes, who pursue the slayers of their own kin. At the trial, Orestes calls upon Apollo to prove that his action was just (609 f.): he does not deny the action, but let Apollo say whether it was just or not. Apollo replies that it was just; this assertion he confirms by his authority as μάντις, and this in turn he justifies by declaring that all his utterances as μάντις are dictated by the commands of Zeus. What Zeus orders must be just; and he calls upon the jury to follow Zeus' will (614 f.). The Chorus asks whether Zeus really can have commanded the oracle to order Orestes to kill his mother; and Apollo replies by explaining that the murder of a woman is a much less serious matter than the treacherous killing of a great prince and warrior. 'As you make it out', reply the Chorus, 'Zeus reckons more of a father's death than of a mother's'; but Zeus threw his own aged father into chains.' This remark gets Apollo on the raw. With extreme rudeness he points out that prisoners can always be released, but that once a man is dead, he cannot be recalled to life.

- 640 ff.:      Χο. πατὴρς προτιμᾷ Ζεὺς μόρον τῷ σὺ λόγῳ.  
αὐτὸς δ' ἔδρασε πατέρα πρεσβύτην Κρόνον.  
πῶς ταῦτα τούτοις οὐκ ἐναντί', ὡς λέγεις;  
Απ. ὦ παντομισῇ κνώδαλα, στύγη θεῶν,  
πέδας μὲν ἂν λύσειεν, ἔστι τοῦδ' ἄκος,  
καὶ κάρτα πολλή μηχανή λυτήριος.  
ἀνδρὸς δ' ἐπειδὴν αἶμ' ἀνασπάσῃ κόνις  
ἅπαξ θανόντος, οὔτις ἔστ' ἀνάστασις.

When the votes are found to be equal and Athena has to decide the issue, she justifies her acquittal of Orestes on what has always been felt by most people to be a trivial ground; she herself has no mother, and is therefore more sympathetic to the father's rights. In fact, one may well wonder why the Erinyes ever agreed to her appointment as arbiter; for as daughter of Zeus and sister of Apollo she could hardly appear likely to support the ancient powers against the new ones. When the verdict is given, they scarcely accept it with a good grace. Their first impulse is to blast the land of Athens with their power, and only in the face of a direct threat of Zeus' thunder do they at length accept Athene's tempting offer.

826-7:

κάγῳ πέποιθα Ζηνί, καὶ τί δεῖ λέγειν;  
καὶ κληῖδας οἶδα δώματος μόνη θεῶν  
ἐν ᾧ κεραυνὸς ἔστιν ἐσφραγισμένος.

We are commonly told that one of the main purposes of the trilogy is to show how the primitive vengeance of the Erinyes was replaced by the rule of law; but nowhere does the text say that the *Eumenides* are now handing over their functions to the Areopagus. They nowhere promise to give up their present pursuits; and nothing in the play justifies the belief that the transition from the blood-feud to the trial by jury is the main theme of the trilogy.

An examination of the surviving plays apart from the *Prometheus* seems to lead to what many people will find the startling conclusion that Aeschylus' conception of Zeus contains nothing that is new, nothing that is sophisticated, and nothing that is profound. But is this conclusion, after all, surprising? No advanced theology of the kind Aeschylus is so often credited with is found in any Greek author before Plato, a fact which Nilsson (see n. 8) acknowledges with some surprise. If Aeschylus was in truth a solitary pioneer of an advanced theology, one would expect to find Plato acknowledging the fact with gratitude and admiration; but this is not the case. In Books II and III of the *Republic*, Plato delivers a fierce attack upon the famous poets of Greece for the corrupting character of the myths which they relate, and from this attack Aeschylus is far from being exempt. The first charge brought against the poets, that of representing the gods as doing violence to one another, is not specifically levelled against Aeschylus, though the *Prometheus* itself could have furnished an excellent example. But as an instance of the second crime of the poets, that of representing the gods as being responsible for evil, Plato quotes the passage of the *Niobe* which I have mentioned (*Resp.* 380 A); as an instance of the third crime, that of making out the gods as capable of deceit, he quotes the disguise assumed by Hera in the *Semele* (*ib.* 381 D); and at the conclusion of the second book he cites, in summing up his remarks upon this head, Thetis' famous denunciation of Apollo's treachery (*fr.* 350, quoted at 383 A-B). Of the crimes of representing death as terrible and of crediting gods and heroes with unbecoming behaviour or with unbecoming sentiments, Aeschylus is not specifically accused, but it is clear that he would not have been acquitted. Now whatever Aeschylus might have said about the other gods, it is highly probable that if he had put forward a conception of Zeus that verged in places on the severe and abstract monotheism which Plato would have approved, Plato would not have included him in his general condemnation of



the poets. Plato, it is true, had little sense of historical accuracy and was capable of unfairness to his predecessors; but I find it hard to believe he would have been so unfair and unintelligent as this.<sup>33</sup>

It seems possible, and indeed probable, that Plato lumped together Aeschylus with the rest simply because Aeschylus shared the common beliefs and assumptions about the gods. Aeschylus certainly makes Zeus supreme; so is he supreme in Homer. Numerous passages could be cited to show that Aeschylus, just as much as Homer, believed in the efficacy of gods other than Zeus, of Apollo and Athene, Artemis and Ares, Hera and Poseidon; numerous others indicate his belief in the quasi-personal existence of the indeterminate daemonic beings in whom the average Athenian of the fifth century undoubtedly believed, Hybris and Dike, Eris and Koros, Ate and Kratos.<sup>34</sup> His presentation of the gods could not be more patently anthropomorphic. They have mouths, feet, arms and eyes;<sup>35</sup> they have special means of transport and use special weapons.<sup>36</sup> Not only are the other gods often introduced upon the stage, but in the *Psychostasia* Zeus himself figured as a character. The gods constantly pursue mortal women, especially Zeus.<sup>37</sup> If Aeschylus had ever heard of Heraclitus or Xenophanes and their attempts to inculcate a more refined notion of divinity, there is nothing in his works to prove it.<sup>38</sup> Further, there can be little doubt that the view of Zeus and Dike that we find in Aeschylus is not materially different from that of Hesiod. Any advance we do find has appeared well over a hundred years before Aeschylus in the work of another Athenian poet, Solon, who presents the notion of Zeus as the champion of Dike and also the doctrine of κόπος followed by ἀτη or ὕβρις in a form in no important way different from what we find at *Ag.* 750 ff. (see fr. 1 Diehl). Scholars have expended much learning and ingenuity in tracing a gradual development in ethical profundity from Homer to Hesiod, from Hesiod to the lyric poets and early elegists, from lyric to Aeschylus, from Aeschylus to Sophocles. Much of their work has no relation to what is in the texts, but is simply a product of the nineteenth century's insistence upon progress in all matters and its conviction that the power of poetry must reside chiefly, if not wholly, in the ideas, and especially the ethical ideas, which it expresses. In fact, as Plato naturally recognised, the similarity of general outlook between all these poets is far more striking than the differences between them. Almost the only scrap of Aeschylus' talk that has come down to us in the remark (in Athenaeus viii 374 E) that his dramas were 'slices from the great banquets of Homer'. I suggest that this may be a more valuable indication of his poetic views than is commonly supposed.

It is time to return to the problem of the Prometheus trilogy; how are we to reconcile the presentation of Zeus in the surviving play as a harsh and ruthless tyrant, the torturer of the great champion of mankind, with the 'advanced' Zeus we find elsewhere in Aeschylus? Does the rejection of the view that Zeus' character 'developed' in the course of the trilogy oblige us to follow Schmid in believing that the surviving play is not by Aeschylus? If the view of Zeus in Aeschylus for which I have argued is correct, there is no reason to suppose the Zeus of the *P.V.* to be un-aeschylean. Zeus may well honour Dike because she was on his side against his father, and uphold her τιμή among men. But for Zeus himself to feel scruples in dealing with a challenge to his own authority would be quite another thing. Any government maintains justice among its subjects, but few governments refer to the arbitration of abstract justice disputes with powers which have challenged their own authority. Further, one must be on one's guard against supposing that the god who is for mankind is necessarily to be thought of as 'good' and the god who is against mankind is necessarily 'bad'. The fifth-century Zeus was not a democratic god, who could never, for fear of losing his job, do anything not in the best interests of the human race. Hesiod, in whose works Aeschylus' mind was soaked, records without expressing disapproval Zeus' annihilation of at least one of the four successive races of mankind. Nor does he feel the fifth race, to which he himself belongs, to have received particularly favourable treatment at Zeus' hands. The gods, he says (*op.* 42 f.), have concealed from men the means of subsistence (βίος); a man could easily do enough work in a day to keep himself for a year, had Zeus not concealed subsistence from us in his anger against Prometheus. No resentment against Zeus is expressed; that would be presumption, and would invite punishment; but neither is his benevolence stressed. What is emphasised, in

<sup>33</sup> I am grateful to Mr. D. A. Rees for urging upon me the usefulness of this argument from Plato.

<sup>34</sup> All mentions of each god and daimon in Aeschylus are conveniently listed by W. Kausche, *Mythologumena Aeschylea* (*Diss. Philol. Halenses*, IX (1888), pp. 129 f. See Chapter II of W. Kranz, *Stasimon* (pp. 34 f.), entitled 'Die göttliche und die menschliche Welt in der alten Tragödie'.

<sup>35</sup> Mouths, *P.V.* 1032, fr. 350, 5; feet, *Eum.* 294; arms or hands, *Supp.* 313, 1066, *P.V.* 849, *Cho.* 395, fr. 327; eyes, *Supp.* 812-13, *P.V.* 654, 903.

<sup>36</sup> Transport, *P.V.* 287-8, 394-6, *Eum.* 493-5. Weapons, *P.V.* 405, 924-5, *Sept.* 131; Apollo and Artemis must have used arrows in the *Niobe*. Zeus' thunder is mentioned at *Sept.* 255, 429-30, 444-6, 513, 629-30, *P.V.* 358 ff., 667-8, 915 f., 1016, 1043-4, 1061-2, 1082-3, *Eum.* 826-8, fr. 196 in Mette's *Nachtrag zum Supplementum Aeschyleum*.

<sup>37</sup> Apollo pursues Cassandra in the *Agamemnon*; Poseidon

pursues Amymone in the *Amymone*; Boreas doubtless pursued Oreithyia in the *Oreithyia*. Zeus pursues Io in the *Suppliants* and *P.V.*, Europe in the *Kares*, Semele in the *Semele-trilogy*, Danae in the *Perseus-trilogy*, and no doubt Callisto in the *Callisto*.

<sup>38</sup> I do not trouble to show how feeble is the evidence adduced by those who have tried to prove him acquainted with the works of these writers. Wm. Nestle's remarks, in the article already quoted (*Griechische Studien* pp. 122-3), provides a fair specimen. Neither shall I deal with the attempts which some have made, encouraged by a light-hearted remark of Cicero ('Aeschylus, non solum poeta sed etiam Pythagoreus . . .', *Tusc.* ii. 10, 23), to prove Aeschylus a Pythagorean or an Orphic. On the Lycurgus trilogy and its connection with Orpheus, see the sensible remarks of I. M. Linforth (*The Arts of Orpheus*, p. 10).



Hesiod and also in tragedy, is the supremacy of his power and the uselessness of trying to deceive him or in any way resist his will. The whole of Greek poetry from Homer to Sophocles abounds with illustrations of this attitude. The gods by their laws encourage righteousness among men. But they themselves are not obliged to obey those laws, nor should we be reasonable to expect it. The poets talk not of the righteousness of the gods, but of their power, and of their insistence that we be righteous; they insist upon the excellence of their laws, but still more upon the foolishness of trying to oppose their will. There is a sentence in the *Magna Moralia* which well illustrates one facet of this difference: 'It would be odd if anyone were to say he loved Zeus' (*Mgn. Mor.* 1208 B 30 ἄτοπον εἰ τις φαίη φιλεῖν τὸν Δία).<sup>39</sup>

There is no reason why a god whom his worshippers regard with this degree of respectful detachment should not be portrayed by one of them as acting ruthlessly, particularly since in this instance Zeus was defending his own supremacy against a challenge. Zeus has a far closer and more personal relation to the action of this play than he has to that of any other surviving work of Aeschylus. Prometheus is a personal enemy, who has defied Zeus' authority; why should Zeus show him mercy, any more than he showed mercy to any other of his defeated rivals? Of course, it is unreasonable to argue that the Zeus of the *P.V.* is the object of the actual hostility of the poet. Aeschylus is not 'for' or 'against' either Zeus or Prometheus. His immediate purpose is not educative, but dramatic; and he must so hold the balance between the protagonists in the central conflict as to exploit to the full the dramatic possibilities which the story offers. Prometheus is indeed shown with much sympathy; but it does not follow that the audience is meant to regard Zeus with the indignation that we naturally feel. The warnings of the Chorus against excessive αὐθαδία have been seized upon by those who have regarded Prometheus himself as being the guilty party. This is ridiculous; but the warnings do serve to remind us that if Prometheus' attitude is magnificent, it is also unrealistic. And unrealism of this kind, the refusal to recognise one's own helplessness in the face of superior power, is a fault that Aeschylus, like other Greek poets, constantly reprobates. The point is made still clearer by the warning of Oceanus, a character whom some scholars have absurdly misunderstood. He offers to intercede with Zeus, at his own risk (325 f., 337 f.); and Prometheus himself acknowledges that some risk could be involved (340-1). What more he could have done, I cannot see. As his offer is rejected, there is nothing left for him to do but to warn Prometheus to moderate the violence of his utterances, a warning which Prometheus would have done well to heed. For all he gets by his rash defiance is an intensification of his punishment.<sup>40</sup>

With this in mind we return to such positive indications as we possess of how Zeus and Prometheus became reconciled.<sup>41</sup> Among these, the prophecies uttered by Prometheus himself in the *Desmotēs* have a peculiar authority; and if we examine them, we find that again and again Prometheus declares that by his possession of the secret about Thetis he will force Zeus to release him. 'I swear', he says (167 f.), 'that the chief of the blessed ones shall yet have need of me, tortured though I am in mighty fetters, to reveal the new design through which he stands to lose his sceptre and his prerogatives. And never shall he coax me by the honey-tongued incantations of Persuasion, never shall I cower before his harsh threats and reveal this secret, till he release me from those cruel bonds and be willing to render me atonement for this outrage.' 'I know', he says later (185 f.), 'I know that Zeus is hard and keeps Justice by his side; but none the less, soft shall his resolution be when by this means he is crushed; and making smooth his rugged temper, he shall come to union and to friendship with me no less eagerly than I.' Observe that in this early scene with the Chorus, Prometheus says that his knowledge of the secret will force Zeus to release him; this statement he repeats at 524-5. In the final scene (908 f.) he talks of the disaster Zeus will suffer if he makes the fated marriage as though it is bound to overtake him. That, no doubt, is intended as a way of threatening Zeus, who as he must know, will hear his words. For though Prometheus knows that Zeus will eventually release him, Zeus can be made to do this only if he knows of the threat which overhangs him. We know, in any case, that Zeus did not suffer destruction; and we may therefore easily infer that it was by revealing the secret that Prometheus purchased his release. Now if Zeus has in the meantime reformed in character, it is odd that he should need the threat of impending disaster to lead him to pardon his noble adversary; this argument, which I have not before seen used, surely confirms arguments against believing that Prometheus owed his release to a change of heart on the part of Zeus which I set out earlier. The change of attitude by the *Eumenides* is

<sup>39</sup> See E. R. Dodds, *The Greeks and The Irrational*, p. 35, with note. *Ar. Rhet.* 1391 B says that εὐτυχία makes men φιλόθεοι. But the context indicates that this means little more than that they are disposed to accept the decisions of the rulers of the universe in an uncomplaining spirit. 'Of the major Olympians', Dodds writes, 'perhaps only Athena inspired an emotion that could reasonably be described as love.' It is probable that in other cities beside Athens the local tutelary god inspired a similar feeling (Hera at Argos, Apollo at Delphi and Delos, etc.).

<sup>40</sup> Much misunderstanding has been caused by II. 342-3:

μάτην γὰρ οὐδὲν ὠφελεῖν  
ἔμοι πονήσεις, εἴ τι καὶ πονεῖν θέλεις.

Denniston (*G.P.*, p. 303) correctly explains the last phrase as meaning 'if you want to labour'; no doubt of the sincerity of Oceanus' offer is implied.

<sup>41</sup> If there was a god to whom the Greeks assigned a not dissimilar relation of partiality, protection, and patronage towards the whole of humanity, it was Prometheus. And they did not conceive of Prometheus as having any share in the government of the universe. This is the prerogative of Zeus, βιαιῶς σέλιμα σπινθὲν ἤμενος.



indeed a parallel, but one which indicates the opposite of what it has often been supposed to. The Eumenides do not change their character, but they do a deal with Athene, and in consequence their attitude changes. So, I suggest, must Zeus have done a deal with Prometheus, and changed his attitude in consequence.

The deal once concluded, it is likely that the trilogy ended in an atmosphere of mutual concession and reconciliation, such as Heracles was a peculiarly fit person to promote. It may well have been indicated that Zeus would not renew the attempt to destroy the human race which Prometheus (231 f.) has claimed to have frustrated, we do not know how. But in view of the explicit words of Prometheus' prophecy, it seems certain that Prometheus' possession of the secret regarding Zeus' dangerous marriage must have played an indispensable part in making this reconciliation possible. Why was this motive necessary to the poet? Firstly, no doubt, because it was part of the myth; secondly, because no Greek of Aeschylus' time or earlier thought of Zeus as having that special relation of patronage and protection towards mankind which the Jews and Christians have assigned to God.<sup>42</sup> Zeus is indeed the champion of Dike, a rough retributive justice; he insists that men, like gods, shall keep his law. This is an important function, and it plays a central part in the action of Aeschylus' plays. That the moral background it provides has great importance, I am very far from wishing to deny. What I do deny is that they contain an *advanced* morality involving an ascription to Zeus of greater power than earlier poets had assigned him and a belief in his regeneration of mankind through suffering. This rude morality serves as an indispensable background to the action. But there is no reason for thinking that the dramatist's main purpose was to commend it to his audience. There was nothing new in it, and among most Athenians there will have been little disposition to dispute it. Aeschylus probably never thought of doubting it, and to that extent his plays may certainly be said to recommend it. I will conclude by quoting some words of Hermann's, set originally in the context of a by no means wholly acceptable polemic against Welcker's reconstruction of the trilogy, but applicable to other opinions besides Welcker's. 'Sed aliud longe est fabulae rationem exponere, aliud explicare consilium poetae, praesertim scaenici, qui non docere, sed placere populo, aut si docere, non docere per aenigmata abstrusae cuiusdam sapientiae, sed per viva constantiae, fortitudinis, animi magnitudinis exempla velit. Ab eo trilogiam factam credere, ut istiusmodi doctrinae philosophicae satisfaceret, alienissimum a sana ratione est, neque utilius, quam quaerere (et quaesiverunt quidam) quomodo in Iovis persona crudelissimi tyranni exemplum proponere potuerit. Neque habuerunt ista apud Graecos offensionem, nec potuerunt habere, ut in religionibus, quae totae ex huiusmodi fabulis essent compositae' (*Opusc.* viii, 255-6).

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<sup>42</sup> Since Aeschylus everywhere works against the background of the system of moral order I have tried to describe, and therefore in a sense may be said to 'teach' that it is true, this statement may be thought to go too far. But the 'teaching' which poets are said by the Aristophanic Aeschylus (*Ran.*

1054-5) to provide was probably held to consist chiefly in the depiction of 'viva constantiae, fortitudinis, animi magnitudinis exempla' (cf. *ib.* 1019-22) and not in the propagation of new religious or philosophical doctrines.



## THE COLOSSUS OF RHODES

I THINK that we all know about the Colossus of Rhodes—one of the Seven Wonders of the World. We remember him from our childhood's days. Was there not some story of a great statue standing astride the harbour of that old-world port—Rhodes, somewhere among the Isles of Greece? And if we look up a Classical Dictionary we shall find him described as being a bronze figure of the sun god, Helios, and as standing 70 cubits, that is, more than 100 feet in height. We read that in the year three hundred and three before Christ, Demetrios Poliorketes, King of Macedon, becoming tired of his long-protracted Siege of Rhodes, returned to Greece, leaving his siege-train behind him. The citizens of Rhodes sold the weapons for 300 talents, and devoted the money to the erection of a statue of their god: a statue, moreover, which they hoped would compare favourably with those of their great ally, Egypt. The citizens of Rhodes called upon the sculptor Chares of Lindos: a town on the island of Rhodes not far from the capital. He was a pupil of the Greek sculptor, Lysippos, who had just constructed at Tarentum a bronze statue of Zeus, about 70 feet in height. Chares undertook the work. The colossus took twelve years to erect, from 292 to 280 B.C. It was thrown down by an earthquake some sixty years later, and remained on the ground, as one of the sights of Rhodes, for nearly 900 years. When the Saracens conquered the island in A.D. 653, their general broke up the figure and sold the bronze to a Jewish dealer. Not a fragment of the Colossus remains to-day, and no complete copy of the figure exists.

Now it happens that by profession I am a sculptor, and naturally the story interested me. I determined to look up the old records, and to find out all I could about this remarkable work, for so far as I could discover, no sculptor had ever yet done that. I thought that a critical examination of the evidence might bring to light some new facts, and I may say here that I was not disappointed, for I was able to find out a great deal that is new about this great figure. And I hope to be able to show you a fairly clear picture of what it really was.

Now the structure of this statue and its method of erection are matters of considerable technical interest. So it is fortunate that in their descriptions of the figure some of the ancient writers unwittingly give us details which throw considerable light upon the sculptor's methods. To begin at the beginning, there is a treatise entitled 'De Septem Orbis Spectaculis', or 'The Seven Wonders of the World', which has been attributed to Philon Byzantinos, a celebrated mechanic, who flourished about the year 146 B.C.<sup>1</sup> Though slightly rhetorical in style, the writer's evident interest in practical technical problems is just what might have been expected from a man with his mathematical and mechanical bent of mind. He shows his interest in the great iron armature which was fixed within and supported the bronze colossus: forged of bars of so great a size as to suggest the employment of Cyclopean force. He discusses the method of constructing the bronze figure, piece by piece, beginning at the feet and adding each newly formed portion, one above the other, so that the whole work rose stage by stage, like a building. He draws attention to the strain on the metal market when a large amount of bronze for the statue was purchased at one time. And to the building of a great ramp of earth by means of which the materials for the statue were raised to the required height above ground. It is because Philon shows this practical grasp of the many problems to be faced that I cannot agree with Fabricius when he 'thinks it impossible that an eminent mechanic like Philon could have written this work'. The writer's use of little unpremeditated phrases—'he first fixed the feet as far as the ankle bones', and 'so the ankles had to be filled in'—suggest that he was in touch with a living tradition, and it is not necessary to attribute to him a date in the fifth or sixth century A.D. as some recent writers have proposed.<sup>2</sup>

The treatise is quite short, it gives a brief description of seven remarkable works which existed at the time. They were: the Pyramids of Egypt; the Pharos or Lighthouse of Alexandria; the Hanging Gardens of Babylon; the Temple of Artemis at Ephesos; the Tomb of Mausollos (the Mausoleum); the gold and ivory statue of Zeus at Olympia; and the Colossus of Rhodes.

Now because he seems to be the earliest writer to mention the Colossus, and from internal evidence also, Philon's account is the most valuable of all the ancient records of the work which have come down to us. Philon writes as a layman on technical processes employed on a work erected more than 100 years before his time, and by methods with which he was not personally familiar. We have no record of the literary sources or local traditions upon which he relied, and we shall find that he was mistaken upon some points. But his general account is

<sup>1</sup> *Philon of Byzantium*  
*De Septem Orbis Spectaculis*, chapter 4. Kindly translated  
for me by Professor R. J. H. Jenkins, of King's College, London.

<sup>2</sup> J. A. Fabricius, *Bibliotheca Graeca* III. 24. 3; W. Kroll, in  
*RE. Philon*, no. 49 (XX, 1941, p. 54).



sound, and we must not allow later accretions to obscure it. Here is Philon's account, slightly shortened:

'... At Rhodes was set up a Colossus 70 cubits high, representing the Sun: for the similitude of the God was known only to his descendants. The artist expended as much bronze on it as seemed likely to create a dearth at the foundries; for the casting of this statue was the world's (triumph) in metal-working. . . . The artist fortified the bronze from within by means of iron scaffolding and squared blocks of stone, whose connecting rods bear witness to hammering of Cyclopean force, and indeed the hidden part of the labour is greater than the visible. . . . He constructed beneath it a base of white marble, and on this, working out the proportion, he first fixed the feet of the Colossus up as far as the ankle-bones, on which the god, 70 cubits high, was to be erected. As the top (*lit.* foot-level) of the base was already so high as to over-top other statues, it was not possible to lift the rest of the statue and place it in position above; so the ankles had to be filled in and the whole to rise on the top (by stages) like a building; for the same reason, in the case of other statues, artists make a model first, then divide it up and cast the parts separately, and lastly join them all together again in erecting it; so here, when the first bit had been cast, the second was modelled on the top of it, and when that had been cast the third was built on to it, and so on, using always the same device of construction. The sculptor then continually piled up round the as yet uncompleted parts of the Colossus a vast mound of earth, which hid the completed parts and allowed the casting of the next stages at ground level. So, going up bit by bit towards the goal of his endeavour, at the expense of 500 talents of bronze and 300 of iron, he made his god equal to the God, raising a work mighty in its boldness; for he gave the world a second Sun to match the first.'

Philon first remarks that so much bronze was expended on the statue that it seemed likely to create a dearth in the local foundries. But later he gives the total weight of the bronze as 500 talents (12½ tons). Now bronze was always a valuable material, and, from early in the Bronze Age, founders' hoards and other evidence, in Western Europe as in China, show that every broken bronze implement or vessel, every scrap of bronze, was searched for and preserved for subsequent re-melting. And the very variable composition of the bronze objects which have survived suggests that many of them were made from fragments of bronze collected haphazard. But the withdrawal from the market of 12½ tons over a period of years should not have caused any serious local shortage. Perhaps the sculptor bought a large supply at one time, so that the work at the foundry should never be delayed by lack of material.

'The artist', says Philon, 'fortified the bronze from within by means of iron scaffolding (or rather "iron stays") and squared blocks of stone whose connecting rods bear witness to hammering of Cyclopean force, and indeed the hidden part of the labour is greater than the visible.' From this passage we learn that inside the figure, from base and ankles upwards, rose columns of well-cut masonry, strengthened by iron bars, the size and thickness of which suggested forging of superhuman power. We shall discuss this point at some length later.

In the next passage Philon tells us that the figure was built up from the top of the base by stages. The feet and ankles first, and then the parts above them, as a building is erected. At each stage of the work, that part of the figure coming immediately above the part already completed, would be modelled in position, perhaps in plaster. (It would be enlarged, of course, from an original model of manageable size.) When the modelling of each stage had been finished, the modelled part would be taken down to the workshops for transformation into bronze. It would then be carried back and fixed into position, and the modelling of the next stage above it undertaken.

Philon goes on to explain that (as in the ancient world it was not possible to lift such heavy masses with any available tackle) the sculptor had a great mound of earth, or ramp, constructed which rose higher and higher as the statue grew, up the slope of which the great masses of stone, iron, and bronze which were required for the statue might be conveyed to their appointed places, high above ground.

Philon's words 'he piled up . . . a vast mound of earth, which hid the completed parts and allowed the casting of the next stages at ground level' may have either of two meanings:

(1) that for each part of the work the furnaces themselves were moved up the mound to such a height that the casting of the new section might take place *in situ*: each new part being cast on to an already completed part of the work; or

(2) that all the casting was done in the foundry at ground level, and each portion when completed was carried up the mound to be fixed in position on the statue.

The first suggested method would be extremely inconvenient and unpractical. It overlooks the fact that after the actual pouring of the molten metal a great deal of work had to be done to the cast before it was ready to be fixed, and most of this work would be done in the bronze-workers' workshop at ground level. Also, as we shall see later, the moving of the furnaces would have been quite unnecessary, as the casting was not done in that manner.



Another account of the figure is that of Strabo, who died in the year A.D. 24.<sup>3</sup> He writes thus of the city of Rhodes:

'... the city ... was embellished with many votive offerings. These are distributed in various places, but the greatest part of them are deposited in the Dionysion and in the Gymnasium. The most remarkable is the Colossus of the Sun, which was 70 cubits in height, the work of Chares of Lindos. It now lies on the ground, having been thrown down by an earthquake, and is broken at the knees. An oracle prohibited it being raised again. This is the most remarkable of the votive offerings, and it is allowed to be one of the seven wonders of the world.'<sup>4</sup>

Strabo's statement that the figure was broken at the knees is of great interest. That it failed there shows that the sculptor had successfully reinforced the ankles: the place where failure perhaps might have been expected. But the earth tremors set up so violent a swaying of the figure that its reinforcement buckled and failed at some point, probably half-way between elbow and ground, and the figure broke at the knees.

Our next writer is Pliny, who died in the year A.D. 79.<sup>5</sup> After describing the Zeus at Tarentum, 40 cubits in height, the work of Lysippos, he goes on to describe the Colossus of Rhodes in these words:

'But that which is by far the most worthy of our admiration, is the colossal statue of the Sun, which stood formerly at Rhodes, and was the work of Chares the Lindian, a pupil of the above-named Lysippos; no less than 70 cubits in height. This statue, fifty-six years after it was erected, was thrown down by an earthquake; but even as it lies, it excites our wonder and admiration. Few men can clasp the thumb in their arms, and its fingers are larger than most statues. Where the limbs are broken asunder, vast caverns are seen yawning in the interior. Within it, too, are to be seen large masses of rock, by the weight of which the artist steadied it while erecting it. It is said that it was twelve years before the statue was completed, and that 300 talents were expended upon it; a sum raised from the engines of warfare which had been abandoned by King Demetrios, when tired of the long-protracted siege of Rhodes.'

Pliny adds little to our knowledge of the work, except that he makes it clear that the masonry filled but part of the interior of the body and limbs.

Our next authority is the Emperor Constantine the Seventh, Porphyrogennetos, who lived from A.D. 905 to 959. In his 'De Administrando Imperio' he writes of Othman, the chief of the Arabs, or Saracens, thus:

'He took Africa by war, and arranged tribute with the Africans, and returned. His general was Mavias, who pulled down the Colossus of Rhodes and took the island of Cyprus and all its cities. ... He demolished the Colossus on it, and a Jewish merchant of Edessa bought it and loaded 900 camels with the bronze of it.'<sup>6</sup>

In another place he writes:

'He took the bronze of it, and carried it over into Syria, and put it up for sale to any who wanted it; and a Hebrew of Edessa bought it and brought it up from the Sea laden on 980 camels.'<sup>6</sup>

You will note that the Emperor uses the words 'pulled down the Colossus', but we had thought of it as already lying on the ground, for Strabo had told us that: 'It now lies on the ground, having been thrown down by an earthquake, and is broken at the knees'.

Our next chronicler is even more explicit. He is Michael the Syrian, who was Patriarch of Antioch during part of the twelfth century.<sup>7</sup> He is writing of the Saracens:

'They went to Rhodes and devastated it. The Colossus of bronze was a fine work, and was reckoned one of the wonders of the world: they set about breaking it up in order to carry off the bronze. It had the pose of a standing man. When they put fire under it they saw that it was fastened to stones in the earth by great bars of iron. With great ropes numerous men pulled on it, and suddenly it turned over and fell to earth. ...'

Michael's statement that 'it turned over and fell to earth' must now be considered in connection with that of Strabo that 'the statue lies on the ground, broken at the knees', and Pliny's words that 'it was thrown down by an earthquake; but even as it lies, it excites our wonder and admiration'. Are these statements irreconcilable? No! I think that we can see what happened. The

<sup>3</sup> Strabo, *The Geography of Strabo*, Book XIV, Chapter 2. 5.

<sup>4</sup> Pliny, *Natural History*, XXXIV, Chapter 18, translated by J. Bostock and H. T. Riley.

<sup>5</sup> Constantine Porphyrogennetos, *Emperor of the East, De Administrando Imperio*, 20-21. Ed. G. F. Moravcsik.

Budapest, 1949. Translated by Professor R. J. H. Jenkins.

<sup>6</sup> Michael I (Kindasi) Patriarch of the Jacobites.

<sup>7</sup> *Chronique de Michel le Syrien, Patriarch of Antioch (1166-99)*, ed. by J.-B. Chabot, Tome II, 1904, fasc. III, Book XI, Chapter 10.



earthquake overturned the Colossus. The stone and iron columns buckled at the knees. The upper part of the statue fell right over, and the head and shoulders reached the ground. But the marble base and the legs up to the knees still stood firmly, with the body and head half hanging from them, half lying on the ground. The iron supports had bent but had not broken, though the stone columns and the bronze sheathing had. The broken figure, supported at the knees, would be 50 or 60 feet high, as tall as a six-storey house. Mavias and his men had to pull it down before they could break it up. He tied strong ropes to the thighs—perhaps he cut some of the irons—and then with a strong pull his men were able to wrench the immense mass free. 'It turned over and fell to earth.' This would be in the year A.D. 653.

Another writer to be mentioned here is J. J. Scaliger, who in 1658 published an edition of Eusebius in which he refers to the Colossus in these words:<sup>8</sup>

'The immense mass of bronze lay on the ground for 977 years until the Arab (general) Mavia, after the capture of Rhodes, sold the Colossus to a merchant, who loaded 900 camels

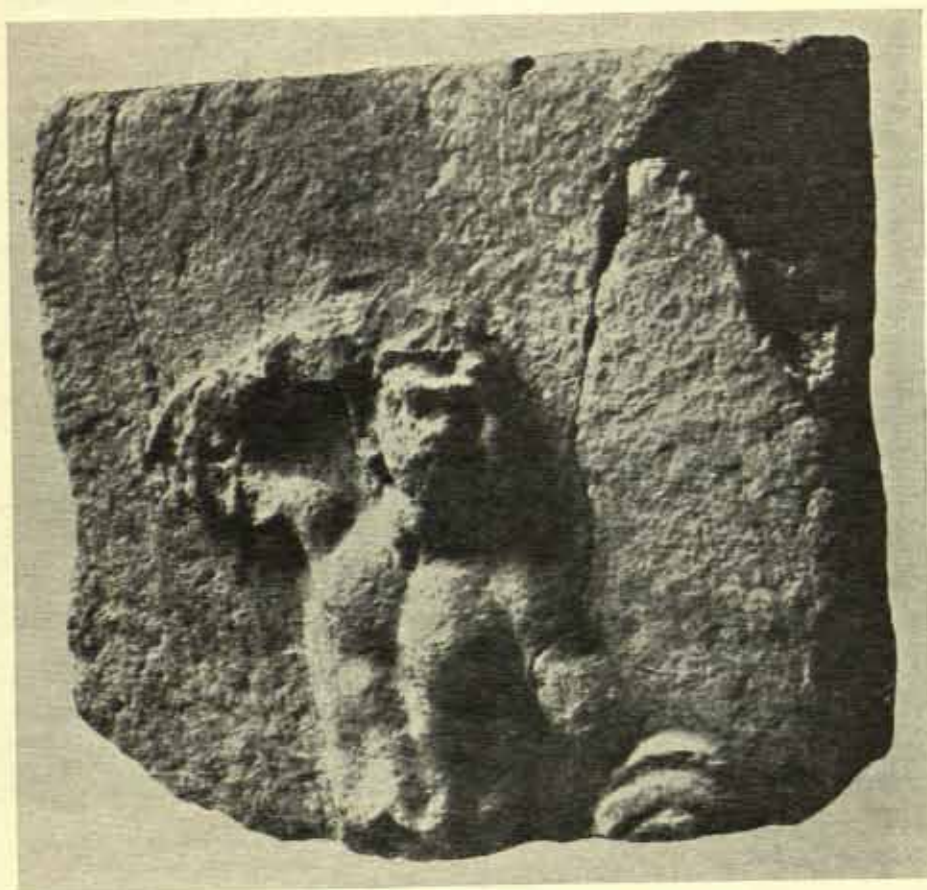


FIG. 1.—RELIEF OF THE COLOSSUS, FOUND IN RHODES.

with the fragments. Now the load of a camel is twice the load of a mule, and is 800 pounds. Therefore, according to this reckoning the material of the Colossus would have amounted to 720,000 pounds.'

But that would mean a total of over 320 tons of bronze employed on the Colossus. You will notice that the weight of bronze reputed to have been used is creeping up. Philon, who gave us the original account of the work, says that 500 talents, or 12½ tons, of bronze, and 300 talents, or 7½ tons, of iron were used on the figure. Scaliger has raised the amount of bronze from 12½ to 320 tons. Another, quite recent, writer gives the number of camel-loads at 900 and the weight of bronze at 900 tons. One ton to one camel! I cannot refrain from giving you one more extract, this time from a recent edition of a Classical Dictionary, originally published 100 years ago. Dr. Lemprière,<sup>9</sup> the compiler, wrote of the Colossus thus:

'Its feet were on two moles which formed the entrance to the harbour, and ships passed full sail between its legs. . . . A winding staircase ran to the top from which could easily be

<sup>8</sup> Eusebius, *Thesaurus Temporum*, 1658, Amsterdam, pp. 137-8, editor Josephi Justi Scaligeri.

<sup>9</sup> Lemprière, John, *Classical Dictionary*, a new edition, revised 1949.



discerned the shores of Syria, and the ships that sailed on the coast of Egypt, by the help of glasses, which were hung on the neck of the Statue.'

But the coast of Egypt was 350 miles away! And the glasses! . . .

We must call a halt somewhere. I have made enquiries, and find that the usual load for a camel such as would be employed in Asia Minor is 4 or 5 hundredweights. So the 12½ tons (28,000 pounds) of bronze, as given by Philon, could have been carried by fifty or sixty camels. If we allow for a further number of bronze statues, which may yet have remained on the island and were collected by Mavias, we may perhaps account for as many as ninety camel-loads of bronze. But the 700, 900, or 30,000 loads, to which the figure was expanded by some later chroniclers, may, most probably, be accounted for by errors of transcription, assisted a little by wonder.

Following the ancient accounts of the Colossus we come at last to a fortunate discovery, such as at times brings joy to the heart of a hard-working antiquary. Some years ago there was discovered in Rhodes a piece of sculpture of the second century B.C.: a relief which gives us an almost contemporary picture of the Colossus itself.<sup>10</sup> (Fig. 1.) The precise date of the relief is perhaps

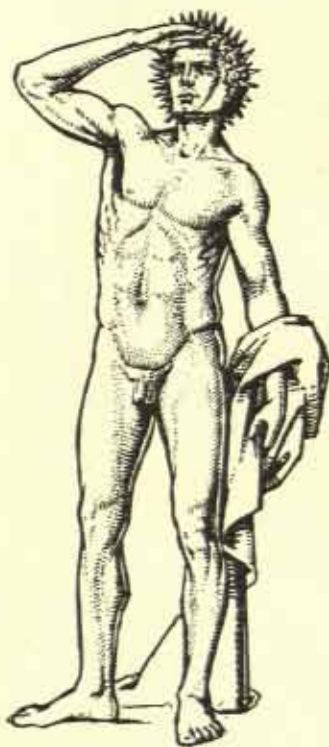


FIG. 2.—SKETCH OF POSE FOR THE COLOSSUS.

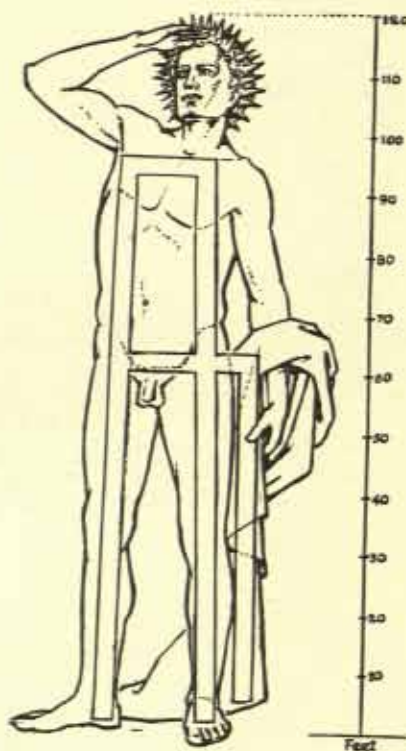


FIG. 3.—THE POSITION OF THE STONE COLUMNS WITHIN THE FIGURE.

(Drawn by Ann Dallas)

difficult to assess. It has in its favour the facts that: (1) it was found in Rhodes; (2) the style is in keeping with the suggested date; and (3) the pose is a probable and suitable one for the purpose.

The pose is a characteristic one for the Sun-god—he is shading his eyes, looking intently at some distant object—in all probability the rising sun (Fig. 2). This is an important observation, for if the statue had been erected at the harbour mouth, facing the sea, then only its back would have been visible from the shore. A glance at the map of Rhodes (Fig. 8) proves that if he faced landwards he would have been gazing intently towards rising ground only a mile away, with nothing important in sight. But if set a little way back from the harbour, facing eastward, the figure could be seen from every side, and the pose shown in the relief would be appropriate.

The relief reveals another feature of immense value to us: a piece of drapery hangs from the left arm. Now why is it present there? We realise at once that, besides the legs, the sculptor would wish to provide the figure with a third point of support to ensure its stability. Later sculptors employed the conventional tree-trunk, or rock, or vase, but Chares preferred this piece of drapery. Seemingly it hangs from the arm, in reality it covered a third column of stone which helped to support the statue.

<sup>10</sup> *Claro Rhodes*, Vol. V, 2, 1932-40, plate II and fig. 15, pp. 24-6.



Fig. 4 shows a contemporary coin of Rhodes, with a head of Helios. A head of this type would probably have been employed for the Colossus.

We have now completed the first part of our enquiry. We have learnt what evidence has survived from the past. Let us now look at the problems that remain.

### THE HEIGHT OF THE COLOSSUS

Practically all the ancient accounts agree that apart from the height of its marble base, the Colossus was a standing figure 70 cubits high. But what was a cubit? In most modern accounts of the work the cubit is reckoned as being equal to 18 inches: making the height of the figure 105 feet. But if we look into the matter we shall find that in the ancient world, though the actual length of the cubit might vary from place to place, nowhere at any time was it 18 inches: so the figure of 105 feet must be abandoned forthwith. In the third century B.C. there were in use a number of different standards of length. There were digits and palms, spans, feet and cubits. And some of the longer measures were divided into halves, quarters, fifths, sixths, sevenths, or even twentieths. Then there were long and short cubits. The late Sir Flinders Petrie, in his study of ancient weights

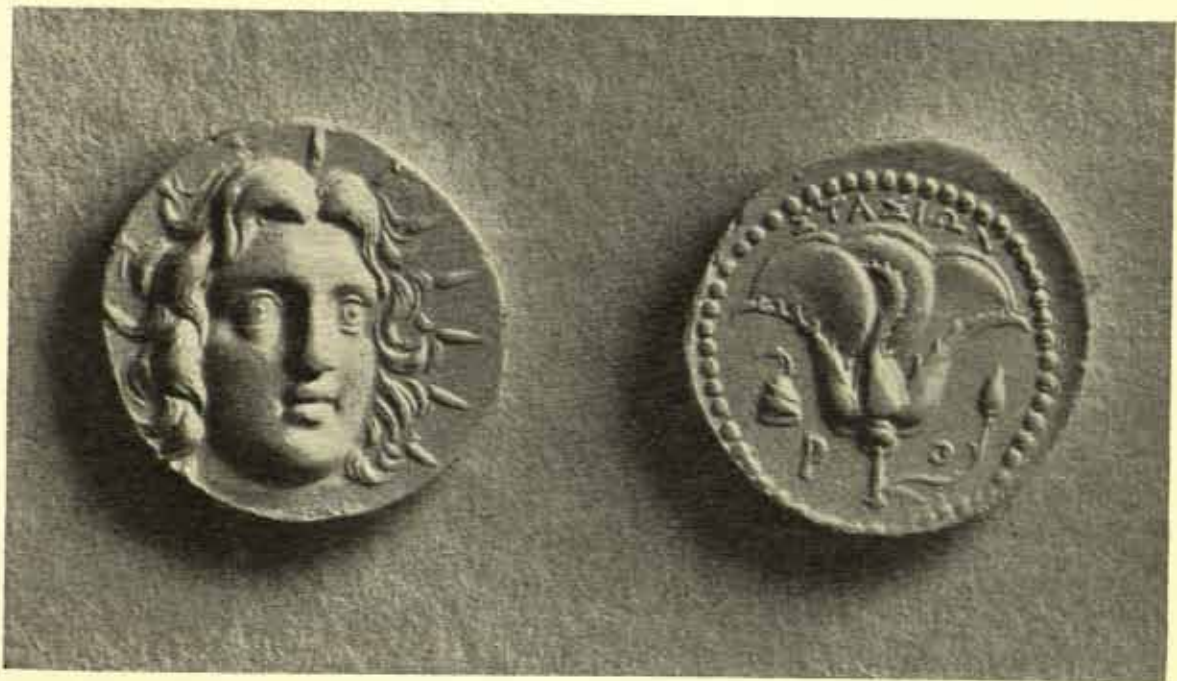


FIG. 4.—COIN WITH HEAD OF HELIOS, THIRD CENTURY B.C.

and measures,<sup>11</sup> made a broad survey of this very difficult material. But no easy or certain solution to the question as to which cubit was actually in use at Rhodes can be found. I have therefore set out here in inches and millimetres the length of a cubit as recovered by actual measurements from a number of buildings of the period in Greece and Asia Minor (Fig. 5). It will be seen that, though there are differences, there is a fair amount of agreement between them.

One of the most important standards was the cubit of Egypt. From pre-dynastic times onward, with but slight variations, the Egyptian royal, or building, cubit had remained at 20.62 inches (524 mm.).<sup>12</sup> If we omit the two measurements taken from buildings in Assyria and Northern Syria, we find that the extreme variation in the length of these cubits is from 20.55 inches in Ephesos to 21.03 at Olympia: a difference of less than half an inch. We cannot be quite sure as to which cubit was employed at Rhodes, but I have taken that of nearby Pergamon as being the most probable. For about 280 B.C. a cubit of 20.7 inches became locally acceptable when adopted by Philetairos, the founder of the Kingdom of Pergamon, as the standard cubit for use in his realm. Its employment would make the height of the Colossus a little over 120 feet, and I have adopted that figure here.

We will consider next:

<sup>11</sup> Flinders Petrie, *Weights and Measures*.

<sup>12</sup> The late Mr. F. G. Skinner, Keeper of the Department of Metrology, Science Museum, South Kensington, London,

wrote two authoritative articles on the history of measures and of weights for *Chambers Encyclopaedia*, published in 1950. He defined the Egyptian Royal Cubit as  $20.62 \pm 0.2$  inches.



## (1) THE THICKNESS OF BRONZE EMPLOYED IN THE COLOSSUS

Philon<sup>1</sup> tells us that its weight was 500 talents,<sup>13</sup> or  $12\frac{1}{2}$  tons, and the amount of iron 300 talents, or  $7\frac{1}{2}$  tons. Remembering that the amount of copper employed in the Statue of Liberty in New York Harbour (a figure of about the same height) was 100 tons, the amount of bronze said to be employed seems to be very small, and the amount of iron proportionally large. Can we check these figures? Let us consider a smaller figure first. A modern bronze-casting for a life-sized, nude, standing athlete may average  $\frac{3}{16}$  inch in thickness, though it may be of stouter material at the ankles and at other places liable to strain, and it may have strong iron reinforcement within. The weight of such a statue will be about 3 hundredweights.

When I began to examine this question I considered: 'Can I discover what is the surface-area of a standing man 6 feet high?' and I tried to reckon up the surface-area of my own body. But a book on human physiology gave me a figure of 20.2 square feet for a 6-foot high athlete.<sup>14</sup> This area is equal to that of a cylinder (including its ends) measuring 0.989 foot in diameter, and 6 feet

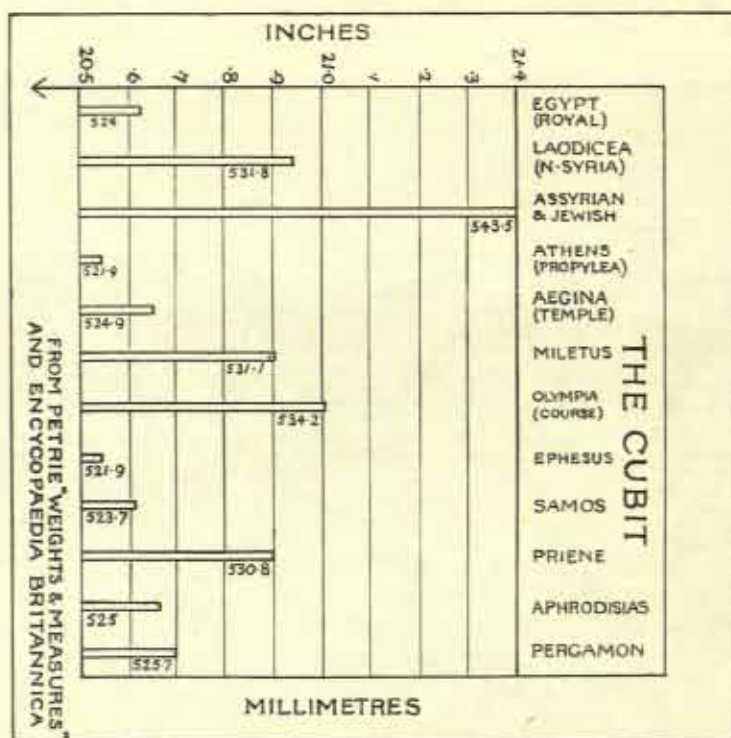


FIG. 5.—THE CUBIT.

high. So the ratio of the height to the diameter of the cylinder is 6.06 to 1. Taking the same proportions, let us see how these measurements would work out for the Colossus. With the height taken at 120 feet, and dividing that figure by 6.06, we find that the cylinder would be 19.78 feet in diameter. The surface-area of such a cylinder, including its ends, is 8,067 square feet. So that is the estimated area of the external bronze surface of the Colossus. To this area we must add, say, about a quarter more to allow for the bronze employed in the drapery which, as I have shown, fell from the left arm to the ground. This would bring the surface-area of the figure and drapery up to 10,000 square feet.

Next, can we discover the thickness of the bronze actually employed in that figure? We can. A cubic inch of bronze (copper 90, tin 10) weighs 5.08 ounces. A square foot of bronze, 1 inch thick, weighs  $144 \times 5.08$  ounces, or  $45\frac{11}{16}$  pounds. And 10,000 square feet, 1 inch thick, will weigh 456,875 pounds, or nearly 204 tons. But Philon tells us that the bronze in the Colossus weighed only  $12\frac{1}{2}$  tons—about  $\frac{1}{16}$  of 204 tons. So we discover the important fact that the figure was built of bronze plates which measured rather less than  $\frac{1}{16}$  inch in thickness.<sup>15</sup> That is just the thickness of a penny.

This is a new fact, and it carries with it important implications. We realise at once that plates of this thickness would have been far too thin to play any serious part in supporting the weight of

<sup>13</sup> One talent may be taken as weighing 57 pounds.

<sup>14</sup> G. H. Best and N. B. Taylor, *The Physiological Basis of*

*Medical Practice*, 5th edition, p. 619, quoting Du Bois, *Basal Metabolism*.  
<sup>15</sup> Or 0.061 inch, or 1.5 mm.



this immense figure, and we are able to appreciate the function of the  $7\frac{1}{2}$  tons of iron. It was employed to reinforce the masonry within the figure, and to support the sheets of bronze of which it was constructed. If the traditional figures for the weight of metal are correct, the Colossus was not built of bronze sections cast to shape, as has been believed hitherto, but it was made of comparatively thin plates, beaten to shape. Thin, but stout enough to serve their purpose. And we learn that the stability of the figure depended solely upon its core of reinforced masonry.

We need have no occasion for surprise at this discovery, for the fifth-century sculptors had done pioneer work by building great statues from thin plates. Sometimes these were fastened to a wooden armature, as in the work of Pheidias at Athens and Olympia. But on other occasions probably the supports were of iron.

At this point let us consider some of the further problems which must have presented themselves to Chares, the sculptor.

There would be:

(1) The design of the figure and its base, and the production of the small-scale model (perhaps 6 or 8 feet high), from which the great figure might be enlarged; together with consideration of the effect of perspective on those parts of a figure which would soar so far above eye-level. And questions as to the manner in which the design and pose would be affected by measures taken to ensure stability against wind pressure and gravity. These constituted a group of aesthetic problems.

(2) The physical structure of the proposed monument—of stone, iron, and bronze; and the problems of its stability against gravity and wind pressure. A group of civil-engineering problems.

(3) The selection of a suitable site. The final choice of this would be made after consultation with the citizens of Rhodes.

(4) The design and construction of the chassis, with its necessary scaffolding, by means of which the form of the great figure might be accurately enlarged from the scale model.

(5) The erection of a moulders' shop for the preparation of the necessary plaster moulds and models; of a foundry for the production of the bronze work; of a workshop in which the bronze plates and their iron supports might be hammered into shape and fitted into their places; a carpenter's workshop, suitably fitted; and the lay-out of a mason's yard for the preparation of the stonework.

(6) Plans for the construction of the great mound or ramp, by means of which the heavy masses of stone and metal might be raised to their positions on the Colossus.

(7) Finally, the problem of cost.

Let us set down here two parallel series of measurements. The first set being the normal measurements of an athlete 6 feet high, such as a Greek sculptor might have chosen as a model,<sup>16</sup> the second set being corresponding figures for a figure 120 feet high, but without making any allowance for perspective:

<i>Athlete</i>	<i>Inches.</i>	<i>Enlarged figure</i>	<i>Feet.</i>
Height . . . . .	72	Height . . . . .	120
Chest, circumference (not expanded) . . . . .	36	Chest (circumference) . . . . .	60
Waist . . . . .	29	Waist . . . . .	48
Thigh . . . . .	22 $\frac{1}{2}$	Thigh . . . . .	37
Ankle . . . . .	9	Ankle . . . . .	15
Biceps . . . . .	14 $\frac{1}{2}$	Biceps . . . . .	24
Wrist . . . . .	6 $\frac{1}{2}$	Wrist . . . . .	11
Height of Head . . . . .	10	Head, height . . . . .	17
Width across shoulders . . . . .	18	Width across shoulders . . . . .	30
Width at armpits . . . . .	12	Width at armpits . . . . .	20
Depth through chest . . . . .	9	Depth through chest . . . . .	15

Now when a figure is seen against a bright sky, the light, crowding past its outlines, tends to make the mass of the work appear more slender than it is in fact. So the sculptor must enlarge his forms lest they appear to be too thin. For example, in an equestrian statue the legs of the horse are often made 2 inches larger than life to compensate for this wastage.

The sculptor Lysippos, in whose studio Chares had served as a pupil, was noted for the grace of his figures and for the excellence of their finish. He worked principally in bronze. In order to make his works more pleasing, he was accustomed to reduce the size of the head of the figure, making it one-eighth instead of about one-seventh of the height, as in a normal figure. But Chares had a very different problem to solve. What would be the effect of perspective on a figure, the head of which would tower perhaps 150 feet above the earth? A small head would be of no use here. He must enlarge, rather than reduce, its relative proportion. So he might make the height of the head 18 feet, or a little more or less, instead of the normal 17 feet, or the 16 feet of Lysippos. Again,

<sup>16</sup> Most of these figures were kindly given me by the editor of the magazine *Health and Strength*.



the size of the ankle of the Colossus, both for optical and for structural reasons, was of critical importance. For unless he were able to introduce some third point of support, such as a tree-trunk, the end of a piece of drapery or some other steadying device, the immense figure would have been balanced solely on its two legs, which at the ankles would measure little more than 4 feet 9 inches in diameter. We know how he overcame that difficulty.

Another problem which must have been frequently in the sculptor's mind was to what extent the stability of the Colossus would be affected by its pose. Let us consider a standing figure in a gale of wind. If the wind blows directly against its chest, the figure is pressed backwards. To counter such a pressure a man would lean slightly towards the wind, shift one foot behind the other, and turn his body sideways to the heaviest blasts. Now the sculptor of the Colossus would have these points in mind. He would consider the direction from which the heaviest gales might be expected to blow, and he might try to so site and pose the Colossus that one foot would point in that direction, with the other foot behind in support, and he would incline the chest so that the full force of the heaviest blasts would not press directly against it. Chares would try to keep his stone columns as evenly balanced as possible, and would rely ultimately upon their iron reinforcement to tie them firmly together.

From Strabo's statement that as a result of the earthquake the figure broke at the knees, we learn that Chares had so reinforced the ankles that even with the excessive strain put upon them by the earthquake they did not fail. But something gave at about knee-level. I am certain, for reasons

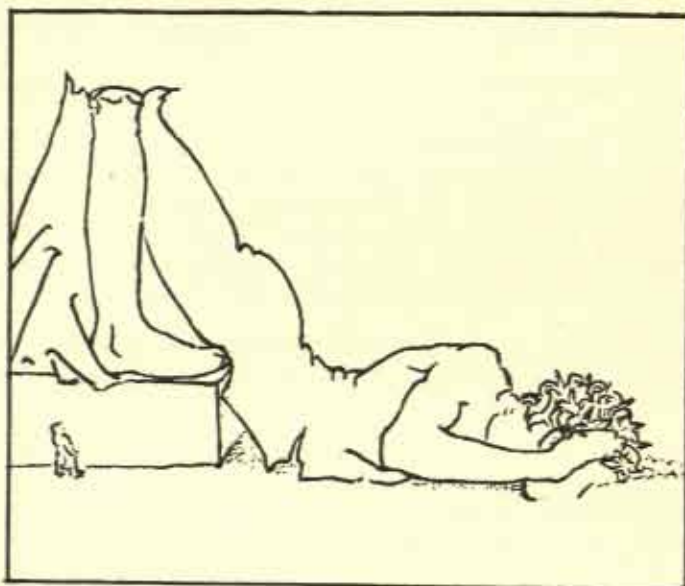


FIG. 6.—THE FALLEN COLOSSUS.

which will be apparent later, that the reinforcing bars did not break, but I believe that the irons and the stone columns buckled there. The failure may have occurred through a defect in the column of drapery, which may have had no lateral support between ground and elbow level, a distance of some 55 feet (Fig. 6). I shall return to this point later.

Now if the feet of a 6-foot-high model were posed about a foot apart (a normal stance), then the feet of the Colossus, in the same pose, would have been 20 feet apart. Chares probably decided to enlarge each ankle to some 5 feet or a little more in diameter, and to strengthen the whole structure with his column of drapery. This would add to his two original points of support a third point, situated perhaps 10 feet away. It would not be in line with the other points of support, but at right angles to them.

The effective area for the support of the figure would thus be greatly enlarged, and with the reinforcing bars which the sculptor proposed to employ, it would seem probable that nothing but an earthquake could bring the Colossus down.

Chares would make his original model for the Helios either life-size, say, 6 feet high, or more likely of heroic size, 6 feet 8 inches high, or, perhaps, to some other measurement which would divide conveniently into 120 feet. This figure he would model, perhaps in clay, and cast in some permanent material. Now Chares was a pupil of Lysippos, one of the greatest of the fourth-century sculptors. Lysippos worked principally in bronze, and was noted for the grace of his figures and the excellence of their finish. He sought also another quality—life. We read that Lysistratos, his brother, to attain this quality in his studies was in the habit of casting parts of the figure in plaster from life; and we remember the life masks which some of the Egyptian sculptors of the Sixth and again of the Eighteenth Dynasty used to take from the living face to assist them in their



work. Now until the last half-century we were accustomed to find in our museums many dull Roman copies of early works from which much of the breath of life was absent. But the discovery, in recent years, of a number of original Greek bronzes has enabled us to realise how vivid a life breathed in the finest works. The Cerigotto Youth is an example of such a bronze, and I think that we should have found in the Colossus something of that spirit. The original model made by Chares would probably be cast in plaster or stucco. He would devote to this part of his work every ounce of professional skill, technical knowledge, and aesthetic imagination of which he was possessed. He knew that in the Colossus every detail in his plaster model would be increased to many times its original size, and that any mistake would have disastrous results.

Let us now consider the dimensions of the base of the statue. Chares would bring to the problem of its height this consideration. The base must be high enough to lift the feet of the figure well above the surrounding buildings. For one should be able to see the whole figure from all sides. Yet the base must not be so high as to interfere with the proportions of the figure. We read that the base of the Colossus was higher than the other statues about it. Chares probably decided to make it 20 or 25 feet high, and tried the effect of a base of that proportion on the scale model.

## (2) THE PHYSICAL STRUCTURE OF THE PROPOSED FIGURE

The Colossus would measure some 60 feet round the chest. It might be 30 feet across the shoulders and 20 feet across the armpits. Naturally, the wind pressure upon such a surface would be very great, and the weight of the figure considerable. So Chares had to discover whether it would be technically possible to introduce into the ankles of the figure and its supporting drapery sufficient masonry, with iron reinforcement, to uphold the weight of the Colossus and to provide sufficient rigidity to withstand a gale. The actual area of support, at ankle level, might be about 20 feet (stride), plus (half of each ankle) another 5 feet, total 25 feet; by (drapery) 10 feet, plus (ankle) 5 feet, total 15 feet. The extreme limits of support at the level of the ankles, therefore, would be a triangle measuring about 25 feet, by 15 feet, by about 29 feet. And we have to consider a possible 80-mile-an-hour gale pressing upon the figure, which rose 110 feet above the ankles.

Now in modern practice a core such as that required for this figure might be designed in terms of reinforced concrete or in steel. But in Chares' time the Greek builders of temples and monuments seem to have fitted their masonry together without cement or mortar. We may note that Philon seems to have been much impressed by the massive iron supports within the figure, which 'bore witness to hammering of Cyclopean force', and it seemed to him that 'the hidden part of the labour was greater than the visible'.

But, unlike a civil engineer of to-day, Chares would not have available in his studio sets of carefully prepared tables dealing with the strength of materials, wind pressures, and so on. He would have to rely principally on observation, and upon data collected from the experience gained by other workers in his chosen field. I think that he would discuss with the most experienced smiths that he could find the size of the supporting bars required for his figure, and the best method of joining the parts together. From Philon's account we learn that Chares employed  $7\frac{1}{2}$  tons of iron altogether. If we compare this amount with that which a modern civil engineer might employ, we find that there is a quite remarkable level of agreement between them. That is a comforting thought. The weight of the figure might be distributed fairly evenly between the three points of support. Let us imagine, within the completed figure, two columns of stone, each rising some 90 or 95 feet above the top of the base, each column being some 5 feet in diameter. They would run straightly through the ankles, legs, and body of the figure right up to the neck. These two columns would be joined together at the level of the pelvis and again at shoulder and neck levels by strong masonry links. A third column of stone would stand some 10 feet behind the others and would pass through and support the drapery which fell from the left arm of the figure. This column would be about 60 feet high, and in its turn would be joined to the other columns by a strong stone link.

It has been calculated that an 80-miles-an-hour gale would apply a force of some 24 tons, acting horizontally against an area of the figure centred about the solar plexus, some 80 feet above the top of the base. It would be to counteract such a force that the sculptor would give much of his attention when designing the iron reinforcement of the figure.

Let us consider the iron bars which linked all together. Altogether there might be six principal supports up to the level of the pelvis. These bars might run some 15 feet down into the mass of the base in order to obtain a good anchorage there, and reach in one piece up to the level of the knees. The two bars in each column would be joined together at intervals by iron ties which passed through the stonework. The vertical bars would probably run in grooves up the front and back of the stone column in each leg, and at the ankles would lie just within the bronze sheathing, obtaining lateral support from the sides of the grooves. Chares would not allow any joint in the vertical bars to come near the ankles. Each of these six lowest bars, two in each leg and two in the drapery, might be some 45 feet long, tapering from about 2 square inches at their lowest point in the base to  $4\frac{1}{2}$  square inches at the ankle level and  $3\frac{1}{2}$  square inches near the knee. Above them, to the



level of the pelvis, some 25 feet higher, the bars might taper from the  $3\frac{1}{2}$  square inches at knee level to 3 square inches at the pelvis. At this level the four bars in the legs would be connected with the two bars of similar size from within the drapery, which, like the others, ran 15 feet into the base.

Up to the pelvis level, therefore, we have three columns of stone fastened together at the top by strong masonry links. Each of these links might be formed from two superposed slabs of granite, each about 17 feet long by 5 feet wide and 18 inches thick. They would reach from one masonry column to another and, indeed, the last 5 feet at each end of the slab would form a drum for the column upon which it rested. Two such slabs would be needed to make up the normal 3 feet height of a drum. Each of these slabs would weigh some 10 tons, and would provide the sculptor with some interesting problems when levering it up the long spiral path to the top of the mound, and again across the top of the pit to its final position on the columns.

Also at the pelvis there would be some of the first of those iron connecting links which so impressed Philon as 'bearing witness to hammering of Cyclopean force'. It is probable that these links would be forged with a strong loop at either end which was fastened to a heavy bar, which

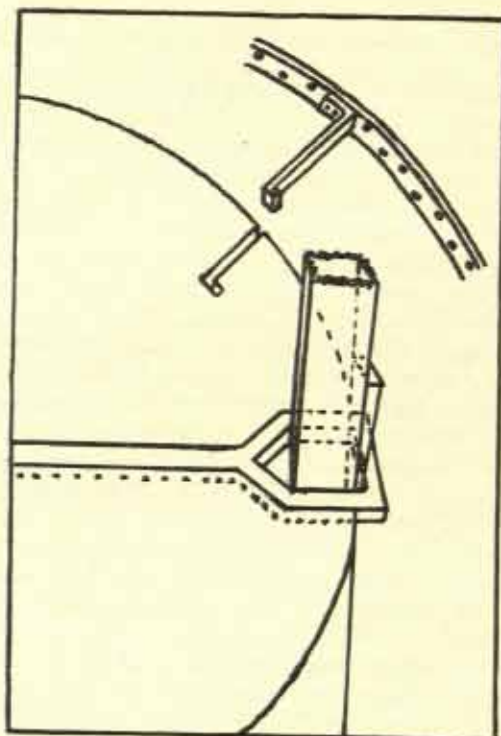


FIG. 7.—PART OF IRON SUPPORTS FOR BRONZE SHEETS. IRON LINKS SUNK IN TOP OF DRUM OF COLUMN, TO CONNECT VERTICAL BARS. WEDGE AT END.

penetrated the column from side to side and was tied firmly to it by iron wedges (Fig. 7). From the level of the pelvis to that of the shoulders and neck the vertical bars might taper from 3 to  $1\frac{1}{4}$  square inches, with strong lateral links.

The weight of the six vertical bars which we have been considering would be about 3 tons in all, leaving some  $4\frac{1}{2}$  tons for the traverse links in the base and in the body, the reinforcement of the head and arms, and for the horizontal supports for the bronze sheathing of the figure. All these might be of iron alone.

Let us now consider the supports for the bronze plates of which the statue itself was composed. We should remember that the individual drums of which the stone columns were composed would be about 5 feet in diameter, and that they had to be raised by rolling or levering them up a sloping path to a very considerable height above the ground. Now such a drum would be fairly heavy. If 3 feet high it would weigh about 4 tons. For the reason given below it is probable that about 3 feet would be the height of drums chosen by Chares for the work.

Surrounding the columns, the sculptor wished to build up a sheath of metal in the form of a man. How could he support the plates of bronze from which the figure was to be made? And what would be a convenient size for the individual plates?

Let us suppose that round the top of each drum the sculptor cut a number of radial grooves, each about 1 inch deep and  $\frac{3}{8}$  inch wide, reaching about 6 inches in from the outside of the drum; the groove being enlarged a little at its innermost end. Now an iron bar measuring 1 inch deep and  $\frac{3}{8}$  inch wide, thickened a little at its inner end by hammering, would be fitted into each groove. It could not be pulled straight out again, being held by its enlarged end. When the next stone



drum was put in place above it, the iron bar would be immovably fixed in position. The outer end of each bar would be turned at right angles, and an iron band, measuring perhaps 1 inch by  $\frac{1}{4}$  inch, bent to the complete section of the figure at that level, riveted to it. So, at the top of each drum there might be seen projecting a row of bars like the spokes of a wheel, and these would be joined together by the iron band, in the position of the felloe or tyre of the wheel. To these strips would be riveted the bronze plates which formed the visible skin of the statue (Fig. 7). How they and the iron strips to which they were fastened were made to the exact size and shape required will be discussed later; here we are considering only the physical structure of the work. It is probable that Chares found that sheets of metal about 3 feet high and perhaps a little longer would be as large as he could conveniently handle for the work. So he might use that sized sheet as a unit.

### (3) THE SITE OF THE COLOSSUS

The actual site of the Colossus in the city of Rhodes is unknown. Let us consider, however, the pose of the figure as shown on the relief. We are dealing with a figure of the sun-god Helios. He is shown peering into the distance. The most probable subject for his search would be the sunrise, or the sunset. But does one shade one's eyes when gazing at the sunset? Hardly. If he were facing the sunrise the pose would be a natural one. And, if the figure stood a little back from the shore, it could be seen well both from the town and from the sea.

At least three alternative positions have been suggested for the Colossus. One suggestion was that the figure stood with legs astride across the mouth of the harbour, and that the ships sailed between its legs (Fig. 8). Now this idea originates in the writings of Vigémère, or of Fabri,<sup>17</sup>

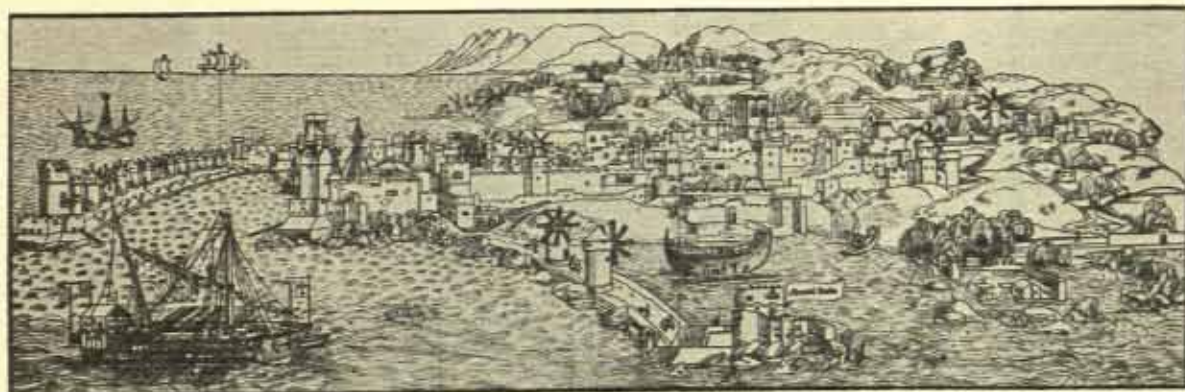


FIG. 8.—VIEW OF THE CITY OF RHODES

who was in Rhodes in the years 1480 and 1483. It has a mediaeval flavour, and it is not mentioned by any early writer. In any case, the plan was an impossible one, for the South Harbour at Rhodes measures some 900 feet across the mouth, and the North Harbour some 600 feet. Again, any site so near to the water would give rise to insuperable difficulties in the design and building of the great earthen mound which, as Philon tells us, was required for the erection of the figure.

Now the builders of temples and other edifices in the third century B.C. had no rope or chain pulleys or other devices capable of lifting heavy weights to a great height. Instead, they were accustomed to build a ramp, or a mound of earth, which was raised from time to time as the work progressed. And eventually it reached to the height of the completed building. So, without the use of cranes or pulleys all the material required for the work could be dragged or rolled up the slope to its position on the building. Philon tells us that such a mound was employed for the construction of the Colossus. It has been calculated that a slope of 1 foot in 10 represents the maximum gradient up which the heavy drums of masonry required for the Colossus could have been rolled or dragged. In all probability the total height of the Colossus with its marble base would have been about 150 feet, so a mound 150 feet high with a spiral path round it, rising at a slope of 1 in 10, and measuring 1,500 feet in length would be needed. The mound would have been about 340 feet in diameter at the base. It would have filled Trafalgar Square, London, and have reached as high as the capital on the Nelson Monument.

The volume of such a mound depends a good deal upon the kind of soil of which it is composed, for the angle of repose varies with different soils. That for sand varies from  $34^{\circ}$  to  $46^{\circ}$ , and other materials between  $40^{\circ}$  and  $45^{\circ}$ . A cone 150 feet high with a slope of  $40^{\circ}$  would contain 6.9 million cubic feet; or with a slope of  $45^{\circ}$  5.3 million cubic feet. This amount of soil had to be excavated, and then moved twice. The maximum number of men working at the site at the commencement,

<sup>17</sup> See H. van Gelder, *Geschichte der Alten Rhodier*, 1900, pp. 382-91. He gives many references to the earlier writers.







ing about 1,200 feet by 800 feet, and it lies alongside the harbour. No more suitable site could be desired. As a further indication, it may be observed that the ancient church of St. John of the Colossus still occupies part of the area.

The sculptor would discuss the site for the statue with the citizens of Rhodes and, that settled, would make arrangements for taking it over and for the erection of the buildings required for his work. When these negotiations had been completed he would set about the work in some such fashion as this.

#### *The Base for the Colossus*

Chares would first assure himself of the soundness of the rock underlying the site upon which he was to build. And then clear down to firm rock an area upon which he could construct the base of the statue. Chares would build up the core of the base to within, say, 15 feet of its top—in fact, up to the level at which the iron reinforcement was to begin. At this level the reinforcement would consist essentially of six large ascending bars with strong horizontal links between them. We do not know whether the bars which the sculptor employed were long enough to reach in one length up to the level of the knees, or whether he employed several shorter lengths one above the other, clamped strongly together. But we may be certain that he would allow no joints to occur just at ankle level. For there he would wish the bars to pass smoothly and comfortably just within the surface, without any unevenness. In any case he would contrive that they spread out fairly widely at their lower extremities so that they might take a good grip on the stonework.

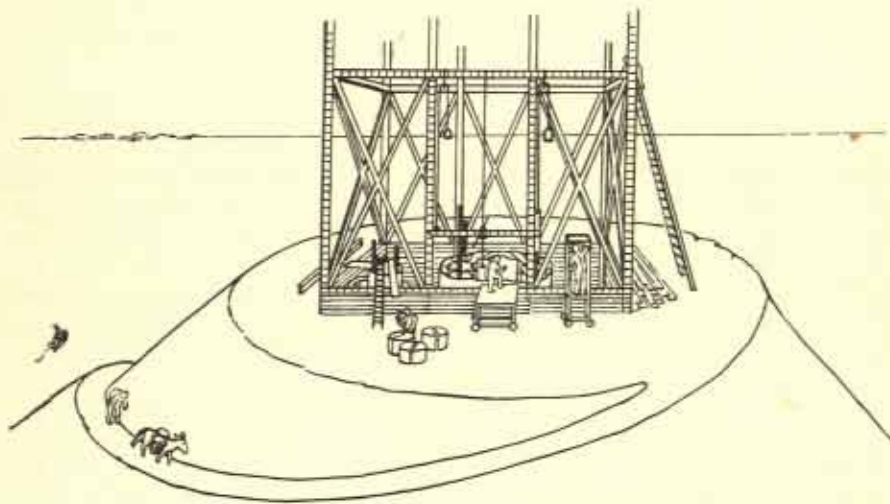


FIG. 10.—THE MOUND AND CHASSIS.

Above the level of the top of the stonework he would cause the bars to approach each other so that four of them came accurately just within the ankles, and the other two passed through the drapery. When this iron reinforcement had been truly placed to at least as high as the calves, the upper 15 feet of the core for the base would be completed. Its marble covering would certainly not be fixed until the whole of the bronze work had been finished. It should be borne in mind that the earthen mound with its sloping ramp would have kept pace with the rise of the base from its foundations, and the masonry could be set without any elaborate scaffolding. The reinforcing bars would be located accurately within the ankles by means of the chassis. And this is an instrument which must now be described.

#### (4) THE CHASSIS

In order to make an enlarged copy of his original model a sculptor may proceed in the following manner. Suppose that this original figure is 6 feet high and that it has been cast in plaster. He constructs round the plaster figure an open rectangular 'chassis' or framework of wood. This he forms of straight, squared bars, perfectly truly planed and with every angle a right-angle. Considerable care is devoted to this structure to ensure that all its outer surface is true and square. The chassis is then placed round the plaster figure, special care being taken to ensure that its sides are placed exactly vertically. Then it is fastened permanently to the model. It will be remembered that in the case of the Colossus the enlarged figure would have many times the dimensions of the original model, so any mistake in the setting of the small chassis might have serious results.

The small chassis is never removed from the model, until the large figure is completed, but the small chassis and model within it may be moved about the site wherever required.



All four vertical sides and the top and bottom rails of the chassis are marked with equal divisions. (A modern sculptor might divide all these bars by inches, or by quarter inches.) Then a similar framework of wood would be built up for the Colossus (Fig. 10), to rise section by section above the top of the base as work on each part of the figure (commencing at the feet) was taken in hand. The large chassis would be marked with a similar set of divisions to that on the small one, though each division would be proportionally larger. On each of the four faces on the large and on the small chassis a plumb line would hang. It could be moved along so as to hang in line with any desired division on the top rail. Also, on each face of the chassis a set-square would be suspended. It could be moved up or down in order to come opposite any part of the figure. Sliding on each set-square would be a fitting through which a small pointed rod, or 'needle', could slide. The needle could move in a horizontal direction only, either directly towards the figure within the chassis, or away from it. Both set-square and needle might be divided, like the other parts of the chassis, into inches and quarter inches, and to correspondingly larger divisions on the great chassis.

Let us suppose that the sculptor wished to find the exact spot in space where the top of the patella of the right knee would come. He would go to that face of the small chassis towards which the knee pointed. He would then slide the set-square, carrying its needle with it, down to such a level that, with suitable forward adjustment, the needle-point could be made to touch the exact spot on the knee that he wished to measure. He would now move the plumb-line till it touched the side of the needle. He could then read off the three measurements: (1) so many vertical divisions above the base; (2) so many divisions laterally from the side of the chassis, as shown by the plumb-line; and (3) so many divisions inward from the face of the chassis. If he now set the needle to similar marks on the corresponding face of the large chassis, then the point of the needle would mark the exact position required. The sculptor could immediately check this position by taking a similar set of measurements from another side of the chassis. The needle thus acts as a completely controlled compass, by means of which any 'point' on the proposed large figure may be accurately fixed in space, however far away it may be situated from the completed parts of the work.

Now, two very important areas which had to be exactly set out quite early in the work were those of the narrowest part of the ankles of the Colossus. They would be situated at a height of about 9 feet above the top of the base. We have seen that Chares had probably decided to make each ankle about 5 feet in diameter. Each stone column must rise from the base and pass within an ankle, its position being carefully fixed by measurements taken with the chassis. The heavy iron reinforcing bars of which we have spoken would pass through the vertical grooves cut in the drums of the columns, and this arrangement would enable the columns to be as large as possible.

### (5) BUILDING THE COLOSSUS

It will be remembered that the Colossus took twelve years to build, an average rise of some 10 or 12 feet a year. That would mean that in that time three or four drums had been successively added to each of the stone columns, and that the horizontal supporting bars for the bronze plates had been fitted into position at the top of each drum before the next drum above it was set in place. If the sculptor ran an iron ribbon between these horizontal bars or spokes, shaped to the exact section of the figure at that level, the hammered bronze plates which formed the external surface of the figure could be riveted to it and to their neighbours on either side before the modelling of the next section above was commenced.

Let us consider for a few moments how the work progressed. Suppose that we are standing on the top of the immense mound of earth, up which we have climbed by a spiral pathway. There is an extensive view all round: the little town of Rhodes lying at our feet with its harbours, and the rocky coast-line stretching away in the distance. Across the sea, some dozen miles away, is the coast of Asia Minor. Close before us rises a rectangular wooden scaffolding, some 30 feet square, like the framework of a building (Fig. 10). Within it we see a screen of bronze which, as a close look tells us, is shaped like part of a man. The lower part of his body and his legs, we find, may be seen in the great pit which, framed by the scaffolding, penetrates the centre of the mound. A platform spans the gap, and we can look down into a great cavern within the body with sides formed by the bronze plates. Tall columns of stone rise from the bottom of the cavern, and from them radiate numberless iron struts which support the bronze walls. Near by on the level top of the mound the original model for the Colossus stands on a bench within its chassis.

Work is in progress, for men have built up above the bronze wall a kind of wooden grating covered with stucco. It forms a panel some 4 feet long, and it seems to be an extension of the modelled surface, rising a few feet higher than the finished portion of the figure. A number of large dots of paint, regularly spaced, appear on its surface. They are 'point marks' or measured points derived from corresponding marks on the original model, and transferred by the aid of the chassis to the great figure. The master sculptor is at work, employing a large rifle with which he works over the modelled surface, modifying it to his liking. He gives an occasional glance at the smaller figure at his side, but within the safe limits of his measured points he is prepared to modify



his modelling to any extent—strengthening a shadow here, truing a line there. When he is satisfied with the work he will give an order, and his assistants will remove the panel from its position on the figure and bear it away to where, at some distance from the foot of the mound, the group of workshops is situated. Should we enter them, we might find a number of models and moulds in plaster or stucco, among which were full-sized models for various parts of the legs and feet of the great figure. Near by, a foundry in which a large bronze plate is being removed from its mould. This was formed from smooth slabs of baked clay. When the plate had cooled enough it would be worked over with scrapers to remove any blisters or other blemishes that might be visible. When no serious defect remained, the plate would be carried into the principal workshop. This would be a big, well-lighted room fitted with long benches and a number of fixed anvils and metal-workers' stakes of various shapes. Resting upon trestles one might see some stucco panels, each about 3 feet high and rather more in length. These would be some of the sculptor's finished models for parts of the figure. The craftsmen would be copying them in bronze. They would take a sheet of bronze as received from the foundry, and with hammers beat it to shape while resting on any stake of a convenient form for that part of the panel. At times they would rest the sheet upon a large sand-bag or upon a cake of lead and strike it with round-faced hammers in order to produce a hollow, or a convex form if seen from the other side of the plate. They would turn the plate about continually, striking it either on the front or back as might be necessary. After being worked thus for a time the plate would become hard, and must be annealed. This work would be done on a hearth with a fire of charcoal blown up by bellows. The plate would be moved about with tongs, so that each part in turn became red hot. Then, when it had been cooled and scrubbed clean with sand and water, it would be taken back to the craftsman. He would compare it with the stucco panel and mark the parts which required modification. Direct hammering would be continued until the master craftsman decided that the work was now far enough advanced for chasing to begin.

A bed of bitumen or pitch would be required as a temporary support during this operation. And with hammers and small chasing punches the finer details of the modelling would be put in. The chasing represents the final stage of the modelling for each part of the figure, though a little touching up by riddle or scraper might eventually follow. When the chasing was completed the panel and the stucco model of which it was a copy would be stood up side by side successively in a number of different lightings, both indoor and out, and the correct modelling of the forms checked before it was finally passed by the sculptor. The work of fitting the plate to its neighbours on either side and into its position on the Colossus followed. Finally, it would be riveted in position to the horizontal bands above and below and to the plates alongside.

It should be remembered that the Colossus measured some 60 feet round the chest, and the curvature of the plates from which that part of the body was made would not depart very far from being a straight line. The thigh was over 11 feet in diameter and the ankle 5. For these curved surfaces the plates could be easily bent before the detailed modelling was commenced. The smiths who produced the body armour and the greaves for the Greek warriors of their time would have found nothing unusually difficult in such work. Indeed, they would have felt quite at home with it. And over them a great sculptor presided.

In the smithy was forged the great iron armature, or scaffolding, which was to come within the figure. It was composed of iron bars whose size and thickness seemed to suggest to Philon 'hammering of Cyclopean force'. Now we have seen that the 'irons', as such reinforcement is called, probably descended some 15 feet into the base, and the ankles of the Colossus came some 9 feet above its top. Chares would probably carry up the bars in one piece as far as the calf of the legs, or possibly to knee-level, before he allowed a joint. But it would not have been possible to make or to fix single iron bars reaching to shoulder level in one stretch. It is true that the Egyptians had cut and erected obelisks measuring 100 feet in height. But consider the difficulty of forging a bar of that length to the exact shape required, and the problem of carrying it up a narrow spiral path to the summit of the mound, and of fixing it in the correct position. The sculptor would certainly have provided joints at convenient intervals in order to simplify the work.

We may ask: 'Could the smiths of that time forge iron bars of the dimensions required?' Certainly they could, and did. And we know that with appliances of the same order the smiths of India made great works of iron which survive to this day. The famous Iron Column of Delhi (Fig. 11) was made about the year A.D. 310. It is of wrought iron and measures 23 feet 8 inches long, with a diameter of 16½ inches at the bottom and 12½ inches at the top. It is not alone. At Dhar, in Central India, are almost contemporary iron beams, broken now, but originally 42 feet long. They were all constructed by welding 'blooms', masses of iron, weighing perhaps 80 lb. each, one after the other on to the end of the column or beam. Some such method of working was employed by the Rhodian smiths.

The forging of the bars destined to support the Colossus, bending them so that they lay just within the surface, say at the ankles, and fitting them into the grooves cut into the columns of stone involved much heavy and careful work, and justify Philon's rhetorical language.

From the relief found at Rhodes, mentioned above, we know that a long fall of drapery hung



from the left arm of the Colossus, and it certainly reached down to foot-level, where its core, the third stone column, rose from the top of the base. If the drapery fell in one length, some 55 feet, without lateral support, we may have in that fact the key to the disaster which overwhelmed the statue. The swaying of the figure caused by the earthquake tremors threw an undue strain upon that member. It buckled, perhaps half-way down, opposite the knees. Or perhaps a reinforcing bar failed. We cannot tell. But the figure, so strongly constructed at the ankles, gave way at a weaker point and fell.

There is a further point concerning the structure of the statue to which attention may now be given. The pose of the right arm, with the hand shading the eyes, brings the thumb almost, or

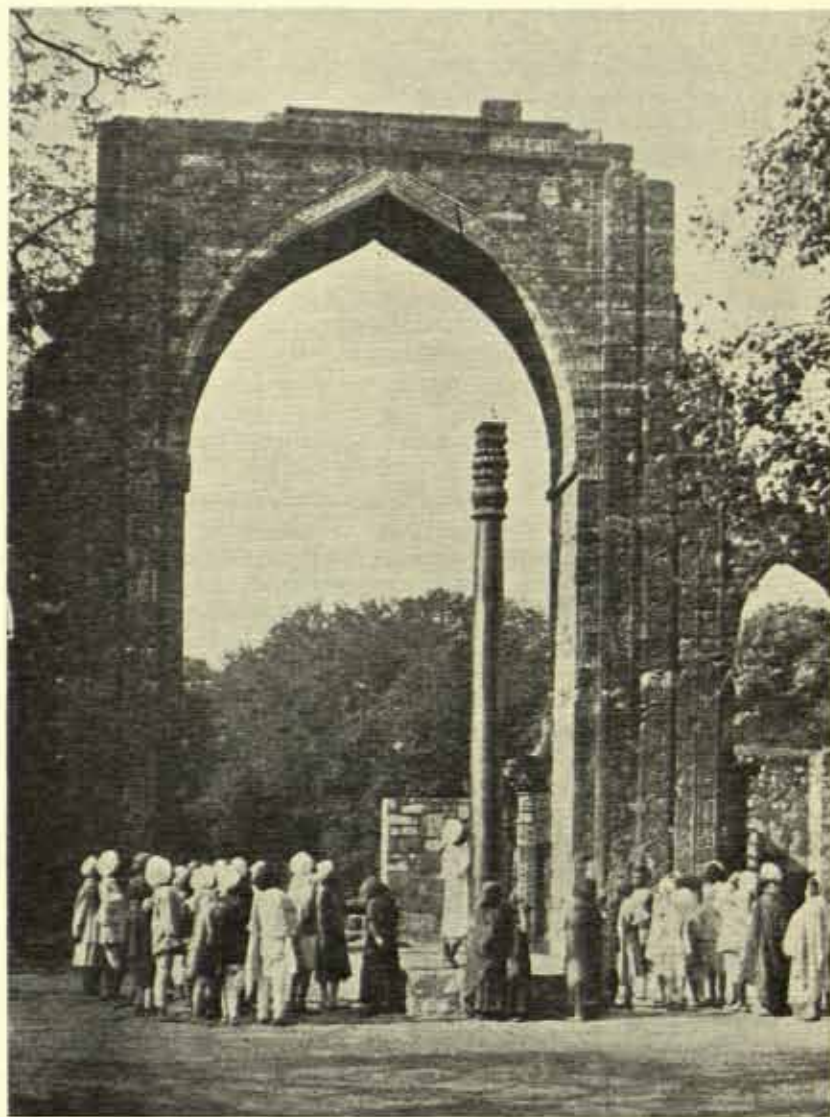


FIG. 11.—THE IRON PILLAR AT DELHI.

quite, into contact with the hair. Why? Consider the structure of the head and arm. They were probably entirely of metal: bronze and iron, without any stone supports. For the two columns of stone which passed through the body would not be needed above shoulder-level. The head and arm could be supported best by irons fastened to the top of the columns, where strong stone links tied them together. But how much stronger would the structure be if at the end of the arm, *i.e.* at the thumb, a strong iron joined it to the head (Fig. 2). Chares would certainly not overlook so obvious a reinforcement of the structure, and we may feel sure that he provided the necessary tie.

#### (6) THE GREAT MOUND

We need not linger over the masons' yard, for its lay-out and the tools employed in it have changed hardly at all in the last 2,000 years. But we must consider the task of erecting the great mound. This, as Philon tells us, was built up gradually round the figure, growing as it grew.



Like the hills of salt which are so characteristic a feature of the landscape along some of the Spanish and French Mediterranean coast-lines, it would have a pathway to the top which coiled round and round it, adding considerably to its width and bulk. The spiral pathway could hardly have been less than 8 feet wide, and there may have been anxious moments when one of the 10-ton links of stone, or a long heavy bar of iron, threatened to take charge. At the top the sculptor would need a space measuring at least 30 feet across to enable him to fix the forearm and hand, and the top of the head. The illustration (Fig. 12) shows the method employed in the early seventh century B.C. by an Assyrian king to move a heavy weight (here an 18-foot-high stone lion). But with the Colossus the problem was made much more difficult by the curving of the pathway round the mound. The building of the mound may have been the work of slave labour, perhaps assisted by some beasts of burden.

#### (7) THE COST OF THE COLOSSUS

But what was the cost of the Colossus? We are told that the expense was 300 talents. Now a talent was equal to 6,000 drachmas. So 300 talents were equal to 1,800,000 drachmas. At Eleusis about the year 330 B.C. a skilled man would be paid about 2 drachmas a day. Owing to

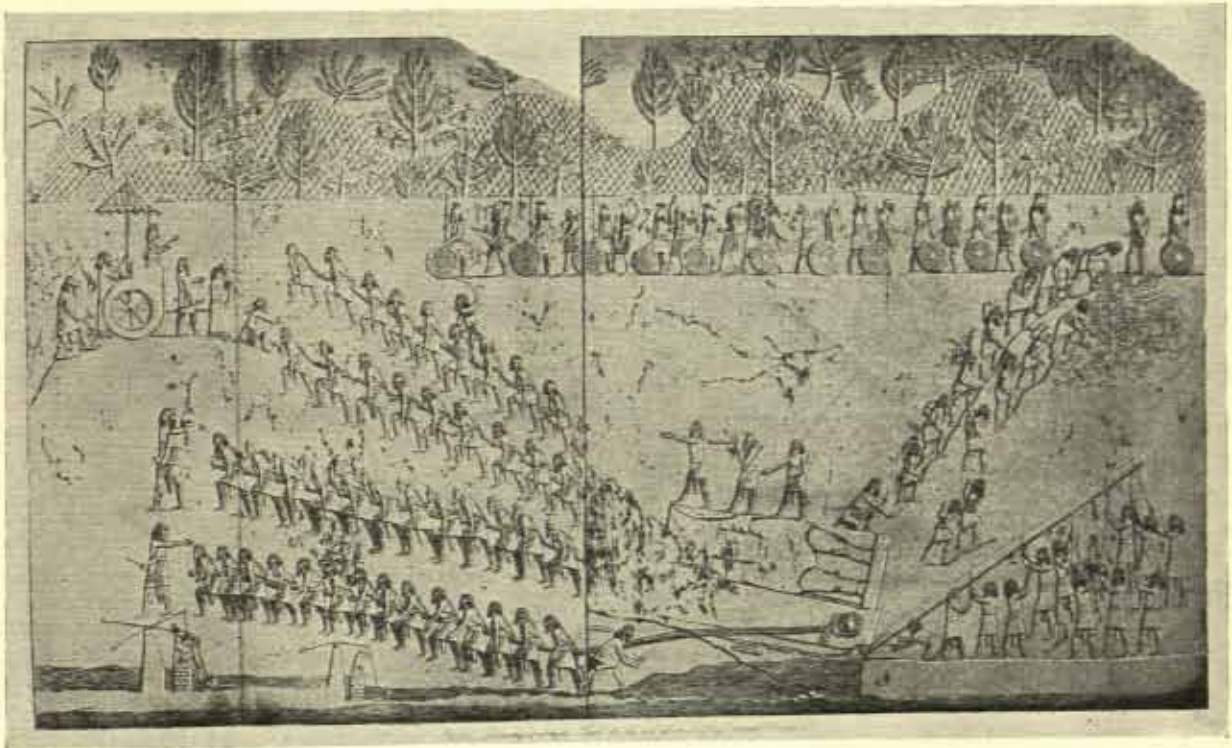


FIG. 12.—AN ASSYRIAN METHOD OF MOVING A COLOSSAL BULL, AND OF BUILDING A MOUND OF EARTH.

the continually varying value of money it is difficult to-day to make a direct comparison, but a skilled man now may be paid £2 a day. However, he spends his money on very different things than did his Hellenic brother. Perhaps we may reckon the cost of the Colossus at half a million pounds sterling, or a million, four hundred thousand dollars.

The cost of the work would be the direct responsibility of the citizens of Rhodes, and would probably be dealt with by the civic officials. It would come under various headings: site, buildings, materials and tools, labour, and so on. The site would be provided by the civic authorities, as also the authority to excavate and remove the earth required for the great mound. Certain buildings for workshops, foundry, storage, etc., would be required near the site, and lodgings for the workers. Excavating tools, and those for stonemasons, smiths, bronze-workers, and carpenters; and the materials themselves: stone, bronze, iron, and wood would be needed. The cost of labour would be the greatest item. Chares would engage the most skilful smiths, founders, bronze-workers, carpenters, and overseers that he could find, and they would be paid at the rate to which their skill entitled them. But the unskilled labour, several hundred men, would probably be provided by the state. They may have been prisoners from the war which had just ended, or camp-followers left behind when King Demetrios withdrew his forces from the island. Their job was but to move earth, and to keep on moving earth for years to come. They were fed and lodged at the expense of the state, and probably felt themselves lucky if they were paid any pocket money.



The sculptor Lysippos, whose pupil Chares was, was noted for the excellent finish of his work. The Colossus also was famous for that reason. As I have said, the fine feeling of life in the Cerigotto bronze gives one a good idea of the quality which would have been found in the Colossus itself.

I am almost at the end of my task. It may have been felt that I have dealt too much with surmises, and that there are so few details of the work upon which we may rely that there can be little solid foundation for my deductions. But, let us consider the picture which the ancient writers and later knowledge have together built up.

Although not a scrap of the original figure remains, we have learnt a great deal about it. The relief found in Rhodes gives us a good idea of its pose, for I accept that relief as an authentic copy of the work. Contemporary coins give the probable appearance of the head. The height was not 105 feet as popularly supposed: it was most probably 120 feet. We have gone a good way towards settling the actual site of the figure. The discovery of the thickness of metal employed on the work is perhaps our greatest surprise, but it puts the work in line with other great figures of the time. We may jettison the 900 camel loads of metal, as we have the 30,000. We have faced the structural problems which, as we know, must have been successfully solved to enable the figure to stand for almost a lifetime. And, though we cannot say that the ancient sculptor's methods were exactly those which I have suggested, I have, at least, indicated the problems to be solved and a means by which the work could be, and probably was, erected. We may think of the citizens of Rhodes gazing with pride at the splendid figure of their god, soaring upwards far above the city—a figure second to none in the whole world. Even in the land of their great ally, Egypt, there was no figure so great as this: none more proudly worshipped. I confess that I feel a little jealous of the citizens of Rhodes.

I take this opportunity of expressing my thanks to the many friends who have assisted me in my work. Particularly to Professor R. J. H. Jenkins, of King's College, University of London, who drew my attention to some of the early references to the Colossus and translated their texts for me; to Dr. Bernard Ashmole and my other colleagues in the British Museum for criticism and help in many problems which have arisen; and to my son, John Gilbert Maryon, civil engineer, whose assistance has been invaluable.<sup>18</sup>

HERBERT MARYON

*British Museum.*

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<sup>18</sup> This paper is based on an account read to the Society of Antiquaries of London on 3rd December, 1953.



## EURIPIDES AND THE ATHENIANS<sup>1</sup>

THE general view of the relationship between Euripides and his fellow citizens which seems at present to hold the field received its most recent, most extreme, and most eloquent expression in the Introduction to Professor D. L. Page's edition of the *Medea*. Here we read: 'Foreshadowed, too, already in *Medea* is that great burden of unpopularity which was to oppress the poet throughout his life. The sequel was the ridicule and hatred which Aristophanes reflects: the climax was his voluntary exile to Macedonia in sorrow and disillusion. Euripides was not the only teacher whom the Athenians persecuted, though they returned to him again and again, admiring while they hated, moved while they mocked and slandered.' After quoting *vv.* 292 ff., where *Medea* speaks of the *φθόνος* incurred by those with a reputation for *σοφία*, Page continues, 'History traces a single undeviating line from this passage of *Medea* through the bitter pages of Aristophanes to the final scene of an old man wandering out into the world friendless and embittered.' A similar picture had already been presented by Wilamowitz and Murray; though Wilamowitz, in his critical account of the life of Euripides published in 1899, thinks that it was chiefly in the last period of his life in Athens, from the production of the *Troades* in 415, that the tension between Euripides and his countrymen became acute, and Murray similarly notes as a significant point the production of the *Troades*, which 'set a flame of discord for ever between his people and himself'.<sup>2</sup> Of his last years in Athens Murray writes, 'Whatever the cause, shortly after the production of the *Orestes* in 408 the old poet's endurance snapped, and at the age apparently of seventy-six, he struck off into voluntary exile.'<sup>3</sup> The general picture, then, is of Euripides spending the last twenty-five years of his life in Athens, especially from 415 onwards, in an atmosphere of increasing isolation, unpopularity, and persecution, shot through with occasional gleams of approbation, until in 408 the tension became unendurable and he left Athens in voluntary exile.

I do not suggest that this account of the matter is radically false; I do suggest that there is exaggeration carried to the point of being positively misleading, and, to say the least, I do not think the evidence entitles us to be as confident and emphatic as the three scholars I have mentioned and some others.

There are three main lines of evidence to be considered: first, the biographical tradition on this point; second, the comparative rarity of Euripides' victories in the dramatic contests; third, the impression derived from the treatment of Euripides in Old Comedy.<sup>4</sup>

Perhaps I had better begin with a brief reminder of the nature of the biographical tradition, in which the items of information, as Nauck judiciously observed, '*nec multa sunt nec satis certa*.' The chief material consists of the *Βίος Εὐριπίδου*, by Satyros, composed in the third century B.C., and the *Γένος καὶ Βίος*, 'Life and Lineage', found in some of our MSS. of Euripides.<sup>5</sup> The fragments of the *Life* by Satyros, discovered at Oxyrhynchus in 1911, are sufficient to give us a fair idea of its own quality and perhaps of the general biographical practice of the period. We know that a good deal of biography was written by members of the Peripatetic school, and it must have become recognised as a regular literary form. As we should expect, the *Life* by Satyros gives the impression of careful writing, and seems to aim at artistic arrangement of the material; in addition, a certain liveliness is imparted by the dialogue form in which it is cast. The literary flavour of the work is increased by the lavish quotations from Euripides and from Comedy, and the account is partly based on the acceptance of the jests of Comedy as historical fact, and on the assumption that autobiographical allusions can be discerned in numerous passages from the plays of Euripides himself.<sup>6</sup> Satyros does not mention any source (except in relation to some stories about Euripides in Macedonia, where he refers to *λόγιοι καὶ γεράτατοι Μακεδόνων*), but such biographical data as cannot have been inferred from literary texts are presumably derived from oral tradition, either

<sup>1</sup> This paper is based on a paper read to the London Classical Society in November, 1954.

<sup>2</sup> *Euripides and his Age*, p. 130.

<sup>3</sup> *Op. cit.*, p. 168.

<sup>4</sup> I do not here discuss the possibility of obtaining any light on our problem from examination of the plays themselves. Euripides, or any other dramatist, could hardly write plays without betraying something of his own inner life and, in the special conditions of Attic tragedy, perhaps something of his relations with the *δῆμος*, but the question when and how he does so is too complicated and controversial to be dealt with in this paper. In general, it seems to me that critics, modern as well as ancient, are too ready to assume that passages in Euripides have not only an extra-dramatic contemporary reference (which is often clear enough) but also a direct autobiographical significance. Thus Wilamowitz (*Einleitung*, p. 15) has no doubt that Fr. 495, by an unknown speaker in an

unknown context, is Euripides' own answer 'mit bittrem worte' to the comic dramatists.

<sup>5</sup> On the ancient biographical tradition, especially on Satyros, see M. Delcourt, *Les Biographies Anciennes d'Euripide*, *L'Antiquité Classique* II (1933), pp. 271-90. To the articles on Satyros mentioned by her and by Schmid-Stählin (*Gr. Lit.* I. 3. 1, p. 309) may be added L. G. St. A. Lewis, *New Chapters in Greek Lit.* (1921), pp. 144-52.

<sup>6</sup> It is, however, important to remember the dialogue form of the work as well as the gaps in our text. Thus M. Delcourt (*op. cit.*, p. 285) is unfair to Satyros in her remarks on the passage (Col. xviii 1) where the speaker cites a few lines of Euripides with the explanation that when the poet says Zeus he means Arche-laos, so that the departure to Macedonia is presumably foreshadowed; for she does not take account of the next speaker, who observes: 'What you say seems to me to be ingenious rather than true.' Some other similar corrections may be lost to us.



directly or through collections of anecdotes or memoirs similar to the fifth-century *Epidemiae* of Ion of Chios.

The γένος is not, of course, a unity, but a collection of biographical notices of different dates, all much later than Satyros. For convenience I refer to the component parts as 1, 2, and 3. γένος 1 (§§ 1 and 2 in Schwartz' edition of the Scholia to Euripides, lines 1-49 in the Budé Euripides) is a biography complete in itself. It has no literary pretensions, and presumably aims at giving as concisely as possible such biographical data as were thought to be of interest to a reader of the plays, though these data are not all well arranged or very critically selected. On a question of chronology the author quotes conflicting views of Philochorus and Eratosthenes, though he may not have been directly acquainted with the work of either.<sup>7</sup> The only other source mentioned in γένος 1 is the comic dramatist Telecleides, of whom two lines are cited to show that the plays of Euripides were partly written by Mnesilochus and Socrates. γένος 2 (Schwartz § 3, Budé text 114-35) is also complete in itself but much shorter, and nearly half of it is concerned with criticism of the poet's style and dramatic technique. The writer is careful to ascribe the story about Euripides' mother to writers of Old Comedy, but for the biographical data there is no reference to any source of information. γένος 3 (the remainder of the whole) is a jumble of anecdotes, of which some may be true, and others are clearly a transference to the life of Euripides of events from the plays, or are based on confusion between the stage of Comedy and real life. The only source mentioned is Hermippus of Smyrna, a Peripatetic biographer, who is cited as authority for the story that after the death of Euripides, Dionysius of Syracuse offered a talent for the poet's lyre and writing materials.

What relationship exists between γένος 1, 2, and 3 and the Life by Satyros remains, I think, in doubt. The references in γένος 2 to a proud and disdainful attitude on the part of Euripides recalls a fragment of Satyros to a somewhat similar effect,<sup>8</sup> and between γένος 3 and some parts of Satyros there is much closer resemblance in content and sometimes in details of wording. It cannot, however, be established that the compiler of γένος 3 knew and used the Life by Satyros, since some of the sources on which Satyros drew may have been available for the compiler of the γένος. All we can be sure of is that in Satyros we have a glimpse of the same sort of biographical tradition at a much earlier stage.

In addition to these main biographical sources we have a chapter in the *Noctes Atticae* of Aulus Gellius, the article in *Suidas* and a notice by Thomas Magister, none of which are of much value for our present problem. Reference will be made to them, and to scattered passages in Aristotle, Plutarch, Athenaeus, and others, in the following attempt to estimate what there is in this biographical tradition to support the impression given by Page and others, what degree of credence it deserves, and what evidence, if any, points in a different direction.

First a minor point: Aulus Gellius cites Philochorus as saying that he has seen the cave of Euripides on Salamis, and Satyros refers to it, telling us that Euripides 'would spend whole days there thinking and writing'. This cave is mentioned again in γένος 3 as a place to which Euripides retired φεύγων τὸν ὄχλον. The fact that such a cave appears to be a common factor in the tradition about many thinkers and writers<sup>9</sup> does not, of course, disprove its existence in any given instance; we need not doubt it for Euripides, but we may doubt whether it necessarily stamps him as a recluse, and whether he really needed it to escape from the ὄχλος, except in the sense that any writer, even an Athenian, might sometimes be glad of a pleasant and secluded spot to work in; even Sophocles cannot have written all his plays in the market-place or at the dinner-table.

We have, however, in another passage of Satyros a specific statement on the relations between Euripides and his fellow-citizens: 'Everybody disliked him, the men on account of his δυσομιλία "unsociableness", the women because of his treatment of them in his plays. He thus incurred great danger from both sexes; for he was prosecuted by Cleon for ἀσέβεια, and attacked by the women at the Thesmophoria.' The origin of the latter story is obvious and does not inspire much confidence in the story of a prosecution by Cleon for impiety, which appears for the first time in Satyros and may also be based on some incident or allusion in Comedy now lost to us.<sup>10</sup> If such a prosecution actually took place it would not be any proof of general unpopularity. On the contrary, if Euripides was accused of impiety by Cleon or anybody else and, unlike some others, acquitted, as we should suppose in the absence of any break in his career and any reference to a penalty imposed, this might be taken as an indication that he was too well established in popular favour for the prosecution to succeed.

Nevertheless, the assertion of the δυσομιλία of Euripides remains, and reappears in later tradition in the adjectives used to describe his appearance and personal characteristics. Thus in γένος 3 he is described as σκυθρωπός, σύννους, αὐστηρός, μισόγελως, and in the article in *Suidas* as ἀμειδής and φεύγων τὰς συνουσίας. In this part of the tradition, then, the notion of the

<sup>7</sup> He mentions, for instance, as a fact that Euripides' mother was a greengrocer, though, according to Aulus Gellius, the improbability of this story had already been demonstrated by Philochorus.

<sup>8</sup> Cf. below p. 89 n. 15.

<sup>9</sup> See Gerstinger, *Wiener Studien*, 38 (1916), pp. 54 ff.

<sup>10</sup> There is a further reference to a charge of impiety brought against Euripides in an unpublished papyrus fragment of the second century A.D. from Oxyrhynchus, which I have seen by courtesy of Prof. E. G. Turner; but there is good reason to think that this fragment describes themes for rhetorical exercises, so that events referred to may be purely imaginary.



δυσκολία of Euripides is established. Where does it come from? Not from the extant plays of Aristophanes and fragments of Old Comedy, the only contemporary evidence. In the *Thesmophoriazusae* Euripides is made to refer to himself as grey-haired and bearded, but otherwise Aristophanes does not make any play with his personal appearance or characteristics, as he does with those of Socrates in the *Clouds*. The impression of Euripides given in later tradition may be partly due to inference from the existence of the cave on Salamis, and partly to the reputation of Euripides as a thinker, since thinkers were apt to be regarded as by definition proud and unsociable. But if this notion grew up in the oral and written tradition of the fourth century, there must have been some further foundation for it. Now Sophocles seems to have been endowed with a particularly attractive personality and a sociable nature; Athenaeus, for instance, describes him as one of the ἀπολαυστικοί, and there is, of course, contemporary evidence to the same effect. We may safely assume that Euripides really was, by contrast with Sophocles, somewhat austere and reserved in manner; but given the existence of some such contrast it was very likely to be exaggerated, possibly in their lifetime, and still more in retrospect, until the antithesis was developed between, on the one hand, the popular Sophocles dancing and dining, and on the other Euripides solitary, sombre, satirical.<sup>11</sup> The increasing popularity of the plays of Euripides in the fourth century might even strengthen the antithesis by drawing attention to the contrast between this posthumous renown and the supposed burden of unpopularity during his life in Athens.

It also seems likely that the departure of Euripides to Macedon, which Murray calls 'voluntary exile', both for ancient biographers and perhaps for some modern critics has cast its shadow back over the preceding years. In Satyros (col. xv 1) the chief speaker describes Euripides as grieved at the ἐπιχώριος φθόνος of his fellow-citizens and annoyed at being associated with (or in competition with) inferior dramatists. There is a gap in the text here, but it seems likely that these feelings are given as the reason for Euripides' departure from Athens, which is alluded to shortly afterwards (col. xvii 1). A little later (col. xix 1) the chief character speaks with indignation of how the Athenians were left behind by the Macedonians and Sicilians in their recognition of the greatness of Euripides. There is, of course, no doubt that soon after the production of the *Orestes* in 408 Euripides left Athens and did not return. On the assumption that he was becoming increasingly unpopular and persecuted in Athens, especially from 415 onwards, this event might naturally be regarded as marking the point when the tension became unendurable; but it cannot in itself be taken as a sure indication of years of growing hostility. Philodemus, writing in the first century B.C., has a passage referring to someone who departed to the court of Archelaus sorrowing because almost everyone rejoiced at his misfortune.<sup>12</sup> This may quite well refer to Euripides, though he was not the only Athenian to join the court of Archelaus, and if so the suggestion is that, at this time at any rate, he was in some trouble and was unpopular. But even if there is some truth in this, it would be rash to generalise too freely about the normal reputation of Euripides in Athens. It is possible that some political or other trouble of which we know nothing may have blown up suddenly and made it advisable for him to leave Athens. It is also possible that he was not actually in any way obliged to leave; that he, like Agathon, Zeuxis, and other distinguished Athenians whom Archelaus gathered about him, welcomed the king's invitation as an opportunity to escape from the war-time hardships of life in Athens;<sup>13</sup> and that although his departure is regularly referred to by scholars as exile, he thought of it as a visit from which he intended to return, though in fact he died in Macedon soon afterwards, just as Aeschylus visited the court of Hiero soon after 458 and did not return.

One further general point about the Life by Satyros is relevant here. It appears that the biographers of the Peripatetic school did not approach their material in a purely objective and detached spirit of historical enquiry, but wrote either in a laudatory or a derogatory spirit, and also aimed at producing an interesting and readable story.<sup>14</sup> The treatment of Socrates by Aristoxenus of Tarentum has something of the nature of a ψόγος; the life of Euripides by Satyros almost amounts to an ἐπαινος: it breathes throughout a spirit of admiration and respect.<sup>15</sup> Now because Euripides was less successful as a dramatist than Sophocles, because he was regularly made the target for the shafts of Comedy, and perhaps also because for whatever reason he did eventually leave Athens for Macedon, it might well seem that the best way to exalt Euripides and present him as a more interesting and romantic figure, was to stress the unworthiness of the Athenians (Satyros himself was not an Athenian and did not write for Athenians) and their lack of appreciation, to invest him with the glamour of the great man honoured elsewhere but misunderstood and persecuted by his own countrymen, who recognised his greatness only after his death. Thus Satyros would seize upon and thereby

<sup>11</sup> It was inevitable that points of contrast and comparison between two such notable contemporaries and rivals should be noted, if not invented; for example, for a contrast on quite different lines, compare the saying recorded by Athenaeus (xiii 603e) that Sophocles was φιλομειραῖς, Euripides φιλογύνης.

<sup>12</sup> *De vitis* X 20 δὲ καὶ φασὶν ἀχθόμενον αὐτὸν ἐπὶ τῷ σχίσθῳ πάντας ἐπιχαίρειν πρὸς Ἀρχέλαον ἀπελθεῖν.

<sup>13</sup> Sophocles was probably invited (*Vit.* 10) but did not go. The occasion of the invitation to the three distinguished Attic dramatists, was perhaps the inauguration by Archelaus at this

time of ἀγῶνες σκηνοί in honour of Zeus and the Muses at Dion in Pieria (Diodorus xvii 16).

<sup>14</sup> Cf. Stuart, *Epochs of Greek and Roman Biography*, pp. 147-54.

<sup>15</sup> In one passage (Fr. 37 Col. 1) where Euripides is described as ἐπὶ τοῖς ἰδίοις ἀγαθοῖς ὑψηλὸς ὢν there is perhaps a slight reminiscence of the Aristotelian μεγαλόμαχος, of whom we are again reminded in γένος 2 in the words περιστάσει τῶν πολλῶν and ὑπερίδων πάντα, which recalls ὑπερόπτης in the Aristotelian description (E.N. 1123b-1124a).



establish more firmly those aspects of current tradition which tended to bear out this picture of Euripides.

The death of Aeschylus in Sicily may have given rise in some quarters to a similar picture of persecution, as in the epigram of Diodorus (*A.P.* vii 40): 'Here lies great Aeschylus, far from his native land of Cecropis;

τίς φθόνος αἶαι,  
Θησείδας ἀγαθῶν ἔγκοτος αἰὲν ἔχει;

Ancient biographers did, of course, ascribe to Aeschylus various motives of pique for his departure from Athens after the *Oresteia*, though modern scholars are sceptical. No doubt, too, any prominent citizen would be at times exposed to φθόνος, but this must be distinguished from a permanent atmosphere of ill-will.

We have been concerned so far mainly with the biographical tradition in Satyros, in γένος 3, and in Suidas. In γένος 2 there is a reference to the studies of Euripides in philosophy, followed by the comment that it was probably on this account that he looked down upon the many. Here we seem to have the traditional figure of the lofty thinker contrasted with the ignorant masses. There is also a reference to the attacks of the comic dramatists as due to φθόνος, and the writer adds that Euripides 'disdaining all this departed to Macedon'. In γένος 1, on the other hand, there is no reference at all to anything amiss in the relationship between Euripides and the Athenians,<sup>16</sup> merely a statement that they erected a cenotaph to him and mourned his death.

My conclusion so far is that the notions of the δυσσουλία of Euripides and of the ill-will of his fellow citizens towards him are undoubtedly present in some aspects of the tradition, but that there are reasons for thinking that the amount of smoke may well be out of all proportion to the size of the fire. It remains to compare what there is in the tradition that points in a different direction.

In the *Life* by Satyros and in Plutarch's *Life* of Nicias the story is told that some of the Athenians captured in Sicily either obtained better treatment or in some cases were even given their freedom because they could recite or sing to their captors passages from the plays of Euripides, and that those who returned to Athens went and thanked Euripides, as well they might. Differences in detail between Satyros and Plutarch suggest the possibility that they derived the story from independent sources, and it seems in any case unlikely that such a story would be a pure fabrication. The number concerned may have been exaggerated (Satyros and Plutarch speak of συχνοί), but even so the impression made in Athens must have been considerable. Our own experience in recent wars has, I think, shown that in war-time such services tend to make an impression out of proportion to their real significance and the numbers involved. It was a fitting corollary to this, and in itself a mark of public esteem, that Euripides, according to Plutarch, was entrusted with the task of composing the ἐπικήδειον for those who fell in Sicily. All this would be round about 412-411, when Euripides is supposed to be becoming more and more lonely and embittered. Of course, public favour is fickle, and we must not make too much of such an incident; neither should it be ignored.

Another episode recorded by Satyros should perhaps be mentioned here. He recounts how 'when Timotheus met with disapproval on account of his innovations in music, and became so depressed that he had decided to kill himself, Euripides alone ridiculed the audience, and perceiving the high quality of Timotheus' art, consoled him in the most encouraging terms, and composed the prelude to the *Persae*, and as a result of a victory Timotheus ceased to be despised'. Wilamowitz gave the date of the first performance as 400-398, and thought it took place at Miletus,<sup>17</sup> and if he is right this, of course, tells strongly against the truth of the tradition. S. E. Bassett, however, has made out a good case for supposing that the first performance was at Athens between 412 and 408,<sup>18</sup> and it seems that the tradition found in Satyros and alluded to by Plutarch<sup>19</sup> should not at any rate be lightly rejected. If there is anything in it, whether Euripides actually composed the prelude or not, the impression it leaves does not quite fit the picture of Euripides as being himself disliked and persecuted, but suggests rather the established and respected dramatist lending a helping hand to a less successful fellow artist.

There is another incident in the tradition from which some inference might be made about the position of Euripides at Athens. In γένος 1 it is said that he went to Magnesia and was honoured there with προξενία and ἀτέλεια. The following sentence implies that this was in 408, when he was on his way to Macedon. It may be assumed that normally a city would try to secure as proxenos a man of good standing in his own city; but in this instance, on the assumption that Euripides was now an exile, we must conclude that proxenia was purely complimentary, or possibly that Euripides was given citizen rights as a compliment, and this became confused with proxenia, which generally carried with it such rights as a reward for services to be rendered. In either case the story would merely illustrate the respect in which Euripides was held abroad. Perhaps, however, we should

<sup>16</sup> Schmid (*Gr. Lit.* I 3. 1. p. 325) is thus incorrect in remarking that all biographies agree on the ill-feeling against Euripides.

<sup>17</sup> Timotheos, *Die Perser* (1903), pp. 64 ff.

<sup>18</sup> *Class. Phil.* xxvi (1931), pp. 153 ff.

<sup>19</sup> *Mor.* 795 D.



consider two other possibilities: that Euripides was made proxenos on the assumption that he would be returning to Athens in due course, or that the compiler of the γένος has gone astray in chronology, and the citizens of Magnesia made Euripides their proxenos in the normal way at some earlier period.<sup>20</sup> Both these possibilities would be more in accordance with the usual meaning of proxenia, and both would imply that Euripides was a person of good standing in his own city.

To conclude this section it is relevant to ask whether Euripides took any part in public life at Athens, apart from his work as dramatist and producer. It may be assumed, as Murray and others note, that he served in the forces like every other male citizen,<sup>21</sup> and silence on this topic in Aristophanes and elsewhere suggests, if anything, that he performed his service without any special distinction or disgrace. It is generally asserted that apart from this he performed no public duty at all, in marked contrast to Sophocles. Aristotle, however, in *Rhetoric* II 6, 20, where he is illustrating a point of psychology applied to rhetoric, refers to 'the answer of Euripides to the Syracusans'.<sup>22</sup> The scholiast says that this answer was given on an occasion when Euripides was sent as an envoy to Syracuse, and the incident, if it is genuine, perhaps took place during the negotiations between Athens and Syracuse between 427 and 415. There is certainly no other reference to this or to any other diplomatic mission of Euripides, and partly for this reason, partly perhaps because the whole thing was thought to be inherently improbable, the passage has been subjected to emendation. It has been suggested that here, as elsewhere, there has been confusion between Euripides and Hyperides, the fourth-century orator; or, alternatively, that Aristotle was referring to another Euripides mentioned in *Ecclesiastus* 825 as an early fourth-century politician, and identified by Wilamowitz with a Heurippides who appears in a fourth-century inscription.<sup>23</sup> On the assumption that Euripides was an embittered recluse who could not have received any such public employment, we must be grateful for any ingenuity that enables us to discount the passage in Aristotle; but if that assumption is questioned we need not be quite so grateful, and we might even reckon with the possibility that it is our Euripides to whom Aristotle refers.<sup>24</sup> Professed experts in rhetoric, like Gorgias, had certainly been employed on diplomatic missions, and in a way it would not be surprising if Euripides *Rhetoricus* was similarly employed. We can hardly reject the possibility merely for lack of confirmation. That Sophocles, for instance, held the office of Hellenotamias is known only from an inscription and is not, as it happens, mentioned at all in any literary source.

In distinguishing lines of evidence I separated the dramatic career of Euripides from the rest of the biographical tradition, partly for convenience, partly because our information on the number of Euripides' productions and victories is probably derived ultimately from official records, and is likely to be more reliable than other biographical items. We need not doubt that the five victories ascribed to him, four in his lifetime and one for a posthumous production, is the correct total. Sophocles won eighteen victories, and though this is out of a total of thirty-one productions against twenty-two of Euripides, the proportion remains far higher and there is no question that Sophocles was far more successful than Euripides in the dramatic contests. The question is whether this relative lack of success, which certainly involved being defeated not only by Sophocles but also by other and inferior dramatists, should be assumed to have induced the sense of bitterness and disillusionment to which scholars refer, and whether it was in itself an indication of general public disapproval of the man or his work or both. This may be to some extent a matter of opinion rather than of argument, but there are two considerations that should not be overlooked.

First, apart from the five victories, we have information, derived probably from the *didascaliae*, of three occasions on which Euripides was second in the contest,<sup>25</sup> and of two or three occasions on which he was third.<sup>26</sup> In the absence of any information about the remaining eleven or twelve productions, it is reasonable to suppose that on some of these occasions he won the second prize,<sup>27</sup> indeed, the assumptions underlying some plays of Aristophanes, which we shall consider later, suggest the probability that he won the second prize on many of those occasions.

Secondly, we should ask whether Euripides was ever refused a chorus.<sup>28</sup> The total number of Euripides' plays is given in the tradition as approximately ninety-two, and *Suidas* adds that he exhibited twenty-two times. This fits well enough, since the four plays left over may, like the *Archelaus*,

<sup>20</sup> Murray, *op. cit.*, p. 169, seems to assume that this was so.

<sup>21</sup> To say, however, that he 'had fought in scores of hand-to-hand battles' (Murray, *op. cit.*, p. 104) seems to be going beyond facts or probabilities.

<sup>22</sup> He does not give the words of the answer, apparently assuming that it was a well-known retort.

<sup>23</sup> *Hermes* 34 (1899), p. 617 and 61 (1926), p. 303; cf. *IG* II i 79. Yet another candidate is a Euripides, father of Xenophon, mentioned in *Thuc.* ii 70, 79, and proposed with hesitation by Sauppe; see Cope, *Rhetoric of Aristotle*, vol. ii, p. 83.

<sup>24</sup> This is the most that can be said on such slight evidence, and I am merely arguing that to dismiss the possibility on the ground that Euripides was ἀπράγμων is to beg the question. Apart from this particular mission, it is perhaps rash to assume that E. remained entirely aloof from all public affairs. The absence of evidence only proves a negative where evidence is to

be expected. It may be safe to conclude that E. never held any important post; but if, for example, he served uneventfully as a βουλευτής and a δικαστής, is there any reason why Aristophanes or anybody else should have mentioned this? We hear of Socrates as a βουλευτής only because his spell of duty was not uneventful.

<sup>25</sup> *Alkestis* (*Arg. Inc.*), *Troades* (*Arg. Arist. ap. Aelian*), *Phoenissae* (*Arg. Arist.*).

<sup>26</sup> *Peliades* (γένος 1), *Medea* (*Arg. Arist.*). There is also a reference in *Suidas* to another 3rd prize, without mention of play or date.

<sup>27</sup> The Argument of Aristophanes says of the *Orestes* τὸ δρᾶμα τῶν ἐπὶ σκηνῆς εὐδοκμοῦντων, but we cannot safely infer from this its fate at the first production.

<sup>28</sup> That is, after his first production. Before their first production most dramatists must have sent in some plays which were returned with the Archon's compliments.



have been produced separately and so omitted from the number of productions given in *Suidas*. At any rate, the inference is that all or nearly all the plays written were produced. It is true that there would presumably be no official record of plays written as distinct from plays produced, so that tradition on this point might be less reliable. Nauck, indeed, seems to think that the number ninety-two must really be derived from the records of plays produced, and on this assumption remarks *si quando Euripides ne tertium quidem praemium consecutus sit, id quod accidisse ei probabile putamus, plus nonaginta duas fabulas ab eo compositas fuisse necesse est*.<sup>29</sup> But this is pure conjecture, and not very plausible. If Euripides had been refused a chorus, this is a matter to which we might have expected to find some reference in Aristophanes or elsewhere, as we do to other dramatists who were thus rejected.<sup>30</sup> Moreover, comparison with Sophocles suggests that ninety-two plays is at any rate likely enough as a total output. Since it is generally agreed that Sophocles was rarely, if ever, refused a chorus, the number of plays ascribed to him—a hundred and twenty-four or thirty-one sets of plays, will be fairly accurate, even if it is really derived from records of production. If we reckon his dramatic career as extending from the date of his first production down to his death, this gives an output of, on the average, one set of plays every two years. Reckoning on the same basis twenty-two sets of plays for Euripides would give almost precisely the same rate of output, and if it is right for Sophocles it may well be about right for Euripides. Thus, although we cannot be sure of precise figures, it seems fair to conclude that Euripides, like Sophocles, was rarely if ever refused a chorus. The official responsible for selecting plays must have been guided partly by the recognised popular estimate of those who submitted plays, and it looks as though long before the end of their careers it was true of Euripides, as of Sophocles, that his work would almost as a matter of course be accepted for production. The point is of some importance, since if Euripides had composed any considerable number of plays which never achieved production, or only on inferior occasions, that might indeed have been a cause of bitterness and frustration. As it was, if we think of his career as one in which he could practically count upon production, in which on three occasions at least and probably many more he won the second prize, and on four occasions won the first prize, should we regard this as a failure? It should be added, perhaps, that although Euripides is far behind Sophocles in the number of his victories, we do not actually know of any other contemporary dramatist who scored more victories than Euripides; but our information is admittedly inadequate. Lastly, I will anticipate the following section by saying that if anything emerges clearly from the *Frogs* of Aristophanes, it is, I believe, the background assumption of the whole play that among contemporary dramatists Sophocles and Euripides were undoubtedly the best. This was, of course, in retrospect, but I think that the evidence of the *Thesmophoriazusae* and other plays supports the view that though Euripides laid himself open to opposition and disapproval, as Sophocles did not, he was nevertheless established as being, after Sophocles, the most distinguished living writer of tragedy.

The only contemporary evidence we have on the relations between Euripides and the Athenians is to be found in the remains of Old Attic Comedy, and it is natural that both ancient and modern critics should be strongly influenced by the impressions they derive from this source. The conclusions reached by Page and other scholars seem to be that Aristophanes and his colleagues attacked Euripides persistently and savagely, and that in so doing they were sustained by general public opinion, and were in fact giving expression to the dislike and disapproval of the majority of the people. Page himself speaks of 'the bitter pages of Aristophanes', and the same adjective is used by Sikes when he writes on the *Frogs*: 'Such is the end of the bitterest attack which any member of the genus irritabile has ever made on a colleague.'<sup>31</sup> Rhys Roberts writes even more passionately: 'One contemporary voice drowns all the rest with its ceaseless torrent of anger and disgust and scorn—Aristophanes.'<sup>32</sup> It is not possible within the limits of this paper to examine in detail the treatment of Euripides by Aristophanes and its implications for the relationship between Euripides and the Athenians, and I cannot now do much more than indicate my own view, with a few words of explanation. This is indeed a matter which could never perhaps be settled entirely by argument, since it depends to some extent on personal impressions of the tone and atmosphere of a play.

In *Wasps*, v 60, Aristophanes mentions as one of the stock ingredients of comedy, along with 'Heracles cheated of his dinner', ἀνασεργασσόμενος Εὐριπίδης. From this alone we might infer that Aristophanes was not the only dramatist to aim his shafts at Euripides. There is, however, little positive evidence of what was said, and we must be content to take our impression from Aristophanes. Some scholars make a clear distinction between earlier plays and the *Frogs*, and would agree that, for example, the scene in the *Acharnians* is good-humoured banter, and that the *Thesmophoriazusae*, as Murray puts it, 'can only be regarded as a tremendous compliment to Euripides', whereas the *Frogs* is regarded as a serious attack.<sup>33</sup> But the more I look at this play, the less ground I see for thinking that Aristophanes intended it in that spirit. The satire is by no means confined to Euripides, but is also directed at Aeschylus, not only in his dramatic technique, but also

<sup>29</sup> Euripides, Teubner Ed. Vol. I xxv.

<sup>30</sup> *Ar. Pax* 801-2.

<sup>31</sup> E. E. Sikes, *The Greek View of Poetry*, p. 60.

<sup>32</sup> W. Rhys Roberts, *Greek Rhetoric and Literary Criticism*, p. 18.

<sup>33</sup> Not, of course, by all scholars; cf. R. E. Wycherley, *Greece and Rome XV* (1946), pp. 98-107.



in the ideals he represents; Aristophanes can hardly have regarded as ideal citizens the stout-hearted but empty-headed warriors proudly presented by Aeschylus in *Frogs* 1069-1073. Very often, of course, the mockery is directed neither at Euripides nor Aeschylus but at Dionysus himself, the god of tragedy, who regularly displays his incapacity for dramatic criticism.<sup>34</sup> Moreover, we must not lose sight of the fact that the whole plot of the *Frogs* assumes that Aeschylus, Sophocles, and Euripides are undoubtedly the three leading dramatists; the inferiority of contemporary dramatists to Euripides is expressly stated, and from the references to them in this play and elsewhere we can see how Aristophanes speaks of those dramatists whom he really did dislike or despise.<sup>35</sup> In the *Frogs* Sophocles is prepared for a possible victory by Euripides over Aeschylus, and in fact the contest is represented as being a very close thing right up to the end. I would suggest that the basic conception of the *Frogs* was not an attack on Euripides, but the idea of exploiting the dramatic possibilities inherent in a clash between two playwrights so conveniently antithetical.<sup>36</sup> This does not, of course, exclude the intention to deliver a few shrewd thrusts at tendencies in contemporary drama which Aristophanes may have deplored on moral or aesthetic grounds. He must have had personal views on politics and drama which were bound to colour his reflection of the contemporary scene, and it is at least arguable that he did in fact think the art of Euripides on the whole inferior to that of Aeschylus or Sophocles; and although there are indications in the plays of Euripides of a political outlook not unlike that of Aristophanes, it may also be true that Aristophanes had grave doubts about the influence on the younger generation of some aspects of Euripidean drama, but this is not to say that the *Frogs* is a 'bitter attack', still less a 'torrent of anger, disgust and scorn'.

For our present purpose it is more important to ask what inference can be drawn from Comedy about the audiences in relation to Euripides. If it is true that the background assumption of the *Frogs* is that among his contemporaries Euripides was second only to Sophocles, we may take it that Aristophanes thought, no doubt rightly, that this assumption would be accepted as reasonable by the audience. This is confirmed by a well-known feature of Aristophanic comedy, the frequent allusions to and parody of lines or scenes from Euripides, allusions which audiences were sometimes apparently expected to appreciate some years after the production of the relevant play. All this seems to argue familiarity with the plays of Euripides not only in Aristophanes but also in at least a fair proportion of his audiences. Is it then natural to suppose that while the plays were well known, much quoted and long remembered, the dramatist himself was normally the object of general dislike and ill-will? As regards detailed criticisms of Euripides by Aristophanes, there must have been considerable differences of opinion among the audience. It is possible that, as a passage in Aristophanes suggests,<sup>37</sup> the most ardent supporters of Euripides might be found among the younger men, and that in some other quarters there was correspondingly persistent antagonism. Generally speaking, however, even if Aristophanes was more hostile to Euripides than I believe him to have been, we must not be too ready to identify the majority of the audience with him. They were accustomed in the atmosphere of Comedy to seeing famous men ridiculed, without taking it all very literally or very seriously. The treatment of Cleon in the *Knights* might with more reason be called an attack, but this play is not generally assumed to have been inspired by or the cause of any general unpopularity of Cleon. The audience were ready to delight in the rough handling of Cleon on the stage, and went away to vote the real Cleon into the office of strategos not long afterwards. No doubt in the *Frogs* and elsewhere they enjoyed the satiric handling of well-known characteristics of Euripides, and liked seeing him, clever and up-to-date as he was, worsted by Aeschylus, the grand old man of tragedy; but I do not think it necessary to suppose that the plays of Aristophanes or other comic dramatists were inspired by or the cause of any general ill-will against him.

Page, Wilamowitz, and Murray certainly present us with a romantic picture of this great poet and dramatist, sitting lonely in his cave by the sea, or moving about the streets of Athens embittered and disillusioned, misunderstood and persecuted, yet evoking at times a strange, unwilling admiration. Then again how dramatic the sudden reversal of opinion after his death, when Sophocles presented his chorus in mourning, a gesture which he presumably expected to be received with sympathy and understanding, not with astonishment or derision; and again when, according to tradition, the Athenians sent to beg from the Macedonians the bones of the man whom they had practically driven out of Athens eighteen months before.

Romantic and dramatic; but can we be sure that it is also true? Or if we consider the tradition as a whole, critically and without any preconceived opinion, it is at least equally compatible with that tradition to suppose that the truth was rather more humdrum: that Euripides was much less successful, less sociable, and less popular than Sophocles, and by comparison showed a certain aloofness and reserve of manner; but that although, like most well-known men, he had his ups and downs in popular favour, in general he was and remained a well-respected fellow citizen, one who

<sup>34</sup> In the verse-weighing scene, for example, he comes to a right conclusion for absurd reasons.

<sup>35</sup> E.g. *Ra.* 86 (Xenocles); *Ach.* 138, *Thesm.* 170 (Theognis); *Pax* 802-17 (Morsimos and Melanthios); *Thesm.* 168-70 (Philocles, Xenocles, Theognis).

<sup>36</sup> Similarly I believe Murray is right in regarding the *Clouds* as a dramatic picture of a clash of humours rather than an attack on Socrates.

<sup>37</sup> *Nub.* 1365-72, 1376-7.



served normally as a soldier, and possibly on a diplomatic mission, but was chiefly famous as being, after Sophocles, the most distinguished dramatist of the day, unorthodox sometimes and disconcerting, one who roused disapproval in some quarters and was fair game for Aristophanes and his colleagues, but a dramatist whose plays everyone wanted to see, and a poet not without honour in his own country, as well as abroad in Sicily, Magnesia, and Macedonia.

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## TWO UNRECOGNISED PTOLEMAIC PAPYRI

### I. THE FRAGMENT OF EURIPIDES' *ORESTES*

THE well-known fragment of a musical score of a chorus from Euripides' *Orestes*, published in 1892 by K. Wessely<sup>1</sup> was dated by its first editor to the first century after Christ. Wessely, who had no dated palaeographical material for comparison, based his dating on external factors. 'The scrap was taken (presumably when unpacked in Vienna, though this is not specifically mentioned) from a layer of papyri which belong to the first century A.D. (the Hadrianic period at the latest), a judgement that I can confirm by means of the dates on some of the pieces should the letter forms not be thought sufficient indication.' Allowing for a certain period of years to elapse before a literary work was set aside, Wessely concluded that 'the evidence permits a date in the time of Augustus'. This conclusion seemed to gain force from a suggestion that the papyrus was contemporary with Dionysius of Halicarnassus' musical score of the *Orestes* (de comp. verb. 11), and has been generally accepted. Nevertheless, the dating is at least two centuries too late. The character of the hand is to a large extent concealed by the photograph (and subsequently apparently it has always been this reproduction that has been reproduced) which accompanied the *editio princeps*. Seen in the original, however, as I was privileged to see it in the summer of 1955, its unmistakable Ptolemaic character thrusts itself on the attention. The mere size of the letters, especially their width, the coarse cut of the writer's pen which can only make thick strokes, the unnecessary horizontal link-strokes found at the top of a vertical hasta (seen clearly in the  $\mu$  of μέγας, l. 2) are among such unmistakable stylistic features. Moreover, some of the letters have a characteristic Ptolemaic shape— $\alpha$ ,  $\kappa$ ,  $\lambda$ ,  $\chi$ , and above all  $\tau$ , which begins with a bold initial upstroke on the left,  $\upsilon$  which has a long, shallow bowl and a leftward curve at the foot of its vertical; to which one is tempted to add the archaic square  $\epsilon$  of the musical notation. Among examples of Ptolemaic calligraphy this hand must take a high place. I know of no precisely similar dated handwriting, for dated literary hands of this period are still rare. But a number of similarities can be seen in any one of the following non-literary texts: P. Cairo Zeno 59532 (epitaph) and 59533 (music),<sup>2</sup> and P.S.I. 379 (letter of 249/8 B.C.),<sup>3</sup> all from the Zenon archive and to be dated about 250 B.C.; P. Teb. 811,<sup>4</sup> a smaller and rougher hand of 165 B.C.; P. London 44,<sup>5</sup> a good documentary hand of 161 B.C. which I should judge later than the *Orestes*. 260 B.C. and 150 B.C. are the extreme limits between which I would assign the date of this papyrus, with a preference for about 200 B.C.<sup>6</sup> On this revised view of its dating, the *Orestes* fragment is even on external grounds to be reckoned among the oldest surviving pieces of Greek music, only the Zenon scrap (P. Cairo Zeno 59533) being perhaps older.

This revised dating tidies up a small matter of notation. In this scrap, a long vowel sung to more than one musical note is written twice:  $\omega\omega\varsigma$  (for  $\omega\varsigma$ ) in l. 6. This duplication of letters (preserved in medieval MSS. for the parody on Euripides  $\epsilon\iota\epsilon\iota\epsilon\iota\lambda\iota\sigma\sigma\epsilon\tau\epsilon$  Aristoph. *Frogs* 1314) is found elsewhere in our surviving fragments of ancient music only in the two Delphic inscriptions, which are certainly of the second century B.C. It may now be held with confidence that the early notation for a syllable sung to more than one note was duplication of the letters, and that the use of a sublinear hyphen is the later one.<sup>7</sup>

The text of Euripides which the papyrus offers differs from the received version in its placing of the parenthetic repeated  $\kappa\alpha\tau\omicron\lambda\omicron\phi\upsilon\rho\omicron\mu\alpha\iota$ . In the manuscripts and editions these words have the status of an independent clause, though the former place them before  $\delta$  μέγας ὄλβος οὐ μόνιμος ἐν βροτοῖς, while most of the latter adopt Kirchhoff's suggestion and put them after this phrase in order to make them occupy precisely the same position in the system as the apparently parallel and responding  $\kappa\alpha\theta\iota\kappa\epsilon\tau\epsilon\upsilon\omicron\mu\alpha\iota$ , l. 324. In the papyrus the words are interjected into the middle of an involved clause in such a way as to separate the subject of the participle  $\pi\omicron\pi\epsilon\upsilon\omega\nu$  from its object  $\alpha\iota\mu\alpha$  (and if  $\delta\varsigma$  is read for  $\delta$ , the following relative from its antecedent.) From the metrical point of view, the transposition of this pair of dochmiacs offers no greater difficulties in obtaining responsion than are involved in obtaining correspondence between ll. 322 and 338 as given in Murray's

<sup>1</sup> *Mitteilungen aus d. Sammlung der Papyrus Erzherzog Rainer*, V, pp. 65–73 (1892). Useful though not exhaustive bibliography of later discussions in R. A. Pack, *The Greek and Latin Literary Texts from Greco-Roman Egypt*, p. 24, No. 300, to which add A. M. Dale, *Lyric Metres of Greek Drama*, pp. 2–3, 194 ff.; E. Martin, *Trois Documents de Musique Grecque*, pp. 14–24; R. P. Winnington-Ingram, *Symbolae Osloenses XXXI* (1955), pp. 65 ff. The most reliable transcript is that of J. F. Mountford, *New Chapters in Greek Literature*, 2nd Series, p. 148.

<sup>2</sup> Plates of these two in Norsa, *Scrittura Letteraria Greca* Tav. 2 and 3.

<sup>3</sup> Plate in Norsa, *Scritt. Doc.* i, Tav. 5.

<sup>4</sup> P. Teb. III, 1, Pl. IV.

<sup>5</sup> I p. 33, plate in Atlas I no. 19.

<sup>6</sup> I was fortunate to be able to discuss the hand with Mlle Claire Préaux and to learn that she agrees with this dating.

<sup>7</sup> Cf. R. P. Winnington-Ingram, *Symbol. OsI.*, XXXI, p. 9.



Oxford text,<sup>8</sup> and supporters of the order found in the papyrus (*e.g.* Crusius<sup>9</sup>) have not been lacking.

Now the revised dating of the fragment here set out means that its aberrant text cannot be dismissed as due to careless copying of the order of lines in the Alexandrian edition. The papyrus must, in fact, be independent of, perhaps prior to, the Alexandrian tradition. An independent tradition may nevertheless still not preserve the right reading, and the calligraphy of the scribe, as appears in the case of the Thucydides text shortly to be considered, need be no guarantee of the correctness of his text. No doubt the reason why editors are reluctant to accept the papyrus order is that they have judged the traditional version to represent, if not what Euripides wrote, at least the considered judgment of the Alexandrian editors. But it is surely difficult to attribute the text of the papyrus to a mere scribal error.<sup>10</sup> Divergencies from the accepted text that are found in early Ptolemaic 'wild' papyri are in general intelligible Greek, that is, they are at least possible variants. Under what conditions, however, is it possible to conceive the displacement of phrases in a lyric text that is 'protected' by musical notation (both of pitch and rhythm)? In using the word 'protected' I am assuming that the notation is not merely an antiquarian apparatus, but represents something that is still practised and appreciated. The answer to a question put in this form must be that groups of words can 'wander' only if their transposition is indifferent from an interpretative, metrical, accentual, and musical point of view. Now, it has already been said that the metrical results of wandering in this case can be tolerated; some scholars, at least, have thought there are no interpretative difficulties; accentual clashes (that is, places where the melodic line falls in spite of the pitch accent) are not involved in the case of this particular phrase, and such examples<sup>11</sup> as do occur elsewhere in the fragment can perhaps be explained, though not very satisfactorily, as due to repetition in the antistrophe of the melody of the strophe. What of the more purely musical aspect? Are we to hold that the nature of the musical phrase was not felt as in itself sufficiently expressive to define the position of a parenthetic interjection such as the repeated *κατολοφύρομαι*? It will not do to explain this lack of expressiveness as due to forcing the melody of the strophe to serve again in the antistrophe, for the strophe itself has just such another parenthesis in the repeated *καθικετεύομαι*. We seem to be forced into the position that in this chorus, at any rate, the movement of the melody was felt as something formal that had no relation to the words. From this position we cannot extricate ourselves by appeal to the order of phrases in the manuscripts, for the parentheses do not occupy the same relative position in strophe and antistrophe there either.

## II. A THIRD-CENTURY B.C. FRAGMENT OF THUCYDIDES

The two scraps of papyrus containing a few lines from Thuc. I, 2 and 28 published as P. Hamb. 163<sup>12</sup> were dated by their editors to the first century A.D. A factor contributing to that dating may have been the belief that these scraps were part of the same roll of Thuc. I as that published by W. G. Waddell in *Ét. de Pap.*<sup>13</sup> 1 (1932), p. 15. The verso, however, of one of these pieces contains elegiac couplets<sup>14</sup> assigned to the third-to-second century B.C., and this aroused suspicion. A photograph kindly supplied through the generosity of Professor B. Snell, Dr. U. Fleischer, and Dr. Voigt of the Hamburger Staats- und Universitätsbibliothek confirms, first, that the proposed identification with the Cairo papyrus cannot be sustained,<sup>15</sup> for the hands are quite different; secondly, that the Hamburg Thucydides belongs unmistakably to the middle of the third century B.C. The handwriting, a beautiful example of calligraphy (See Fig. 1), can be closely paralleled from the following texts: P. Hibeh 1 (Plate I, not later than 240 B.C.); P. Cairo Zenon V 59816 (257 B.C.) and P. Petrie II, 11, 1 (*c.* 260 B.C.); the fragment of Euripides' *Hippolytus* found among Zenon's papers, P. Lit. Lond. 73 (Plate IVB), and is therefore to be dated about 250 B.C. The letters of the Hamburg scribe are distinguished by their bold initial and terminal dots and strokes: τ, γ, even ι

<sup>8</sup> I am indebted to Miss A. M. Dale for the following comment: 'There is a small point here which cannot be pressed at all hard. When Euripides has a dochmiac dimeter he tends (in the great majority of cases, in fact) to use diaeresis; the cases where he does not do so usually take the form either of word-end before the final long of the first dochmiac (μεγάλῳ ἄρῳ: εὐ- μείδης, 321) or the hang-over of one short syllable (ἀλβροῖς ἀλεθρίοισιν-ιν ἐν κόμισσιν, 344). Moreover, in the first of these cases the initial syllable included in the first dochmiac is usually some easily detachable prefix (εὐ- 321, ἀμ- πάλλεσθ' 322). Now the pair

ταναὺν αἰθέρ' : ἀμ- πάλλεσθ' αἵματος  
ματέρος αἶμα : σῆς, ὃ σ' ἀναβαλκεύει

is slightly better matched than

ταναὺν αἰθέρ' : ἀμ- πάλλεσθ' αἵματος  
καθικε τεύομαι : καθικετεύομαι,

But as the quantitative respension is so free anyhow, nothing can be made of this here.'

<sup>9</sup> *Philologus* 52, 179. The argument is: (a) that it is hard to

account for the position of *κατολοφύρομαι* in the papyrus unless it is either right, or mere mechanical failure; (b) that the outburst in an emotionally bracketed parenthesis is in any case more effective.

<sup>10</sup> In that case, has the scribe got both words and musical notation in the wrong order, or have the words and music got out of phase?

<sup>11</sup> There are four such transgressions in this text. See most recently R. P. Winnington-Ingram, *Symbol. Osl. l.c.* p. 65 and n. 3. I am indebted to him for criticism of the statement of my argument in this paragraph.

<sup>12</sup> *Griechische Papyri der Hamburger Staats- und Universitätsbibliothek*, Hamburg 1954. (O. Luschkat was able to see it for his new Teubner *Thucydides*, vol. I (1954), p. 18. He dates it first century A.D., following the Hamburg edd. He is not always correct in recording its readings.)

<sup>13</sup> P. Cairo 47993.

<sup>14</sup> P. 666, publ. as Hamb. 124 in the same volume.

<sup>15</sup> For a photograph of the Cairo papyrus I am grateful to Mr. T. G. H. James and to the Cairo Museum.



show this feature,  $\nu$  has an exaggeratedly long top bowl,  $\mu$  is made in four strokes and is very deep,  $\theta$  is written small and round and with an internal dot, not a cross stroke,  $\omega$  is raised off the line, and its second loop is smaller and higher than its first.  $\alpha$  also has the shape characteristic of the third century B.C., and punctuation by wide spaces in the line<sup>16</sup> is another feature of manuscripts of that date.

These scraps are, therefore, the earliest surviving witness to the text of Thucydides, and a special interest attaches to them in consequence. The scribe, following the idiom of his own day, writes Θετταλία, ἐαυτούς,<sup>17</sup> αἰ not αἰε (just as other papyri and most manuscripts do).<sup>18</sup> Words change their order in c.2.3 Θεττα[λί]α νῦν καλουμένη.<sup>19</sup> τε is misplaced in c.29.3 (ζεῦξαντες τὰς τε παλαιάς, papyrus; ζεύξαντες τε τὰς παλαιάς rightly, Codd.). The scribe falsely simplifies in c.2.3. Πελοπόννησος [τε πλὴν Ἀρκαδι]ας<sup>20</sup> instead of the typically Thucydidean variation with the partitive genitive Πελοποννήσου τε τὰ πολλά. There is worse than this, for διανοίαι is substi-



FIG. 1.

tuted for παρασκευῇ in the phrase οὔτε τῇ ἄλλῃ παρασκευῇ which closes c.2.2. Like the Hamburg editors, I do not understand διανοίᾳ here, but the word cannot be explained as due to a mechanical failure of copying.

In view of these corruptions, a suspicion will intrude that even where the papyrus has a defensible variant, the text may have been manipulated in the interest of clarity. There are two such cases: c.2.2 [ῥηγούμεν]οι [πανταχοῦ ἀν ἐ]πικρατεῖν<sup>21</sup> gives a lucidity to the word order lacking in the manuscripts. ἀνίσταντο three words later (codd. plerique ἀπανίσταντο) is the reading of the second hand of M and the quotations in Dionysius of Halicarnassus,<sup>22</sup> and is in conformity with Thucydidean usage when the sense is 'migrate' or 'be forced to migrate'.<sup>23</sup> It should, however, be noted that the papyrus has the true reading ἀπο[γάγωσι] at c.28.5 and may well have had the same true reading in 28.4.<sup>24</sup> Perhaps therefore it may be allowed to have the true reading elsewhere.

<sup>18</sup> Between διανοίαι and μάλιστα, l. 59 of ed. princeps.

<sup>17</sup> For Egypt, cf. Mayser, *Grammatik* I p. 305.

<sup>18</sup> The authority for restoring *αια* to the text of Thucydides is Marcellinus' *Life*, 52 and the usage of fifth-century inscriptions, not the manuscripts, as the Hamburg editors state.

<sup>18</sup> It might be argued that with this order it is easier to couple the following Βοιωτία with the participle. In any case our scribe seems to have construed Θετταλία, Βοιωτία and Πελοπόννησος as a triplet, cf. following note.

<sup>20</sup> Possibly no weight should be put on a restored passage. But at least the termination -ος of Πελοπόννησος is clear.

<sup>21</sup> The editors' restoration seems the only sensible one.

<sup>22</sup> Pp. 165 and 805 Usener-Radermacher. Dionysius' text is probably not independent of M, and in any case he may be

paraphrasing, cf. his *olόμενοι* for *ἡγούμενοι* just before.

<sup>23</sup> Cf. I, 8, 2 ἀνίστησαν; 12, 3 Βοιωτοὶ ἀναστάντες, 12, 4 ἀνισταμένη; II 14 ἡ ἀνάστασις ἐγένετο.

<sup>24</sup> There can be no doubt that ἀπα[γάγων] is correctly restored in ed. princeps l. 14, but in l. 8 the editors want to restore ἀνά[γων] and give a reading in both places that squares exactly with that of ABEFM. But an estimate of the numbers of letters lost on the left-hand side of this column (assumed to be 14 letters in l. 6, 13 in 7, 13 in 8, 12 in 9, 15 in 10, 14 in 11, 12 in 12) shows a variation between 12 and 15 that is not explained by the line of fracture of the papyrus, and does not allow us to prefer the shorter ἀνά[γων] to ἀπα[γάγων] as the restoration in l. 8.



The papyrus is of interest, not for the text it presents, which is clearly 'wild' and erratic, but for two questions it raises about the tradition. It illustrates forcibly what every editor knows, that Thucydides' text was peculiarly liable to early corruption. Remembering that the unknown and uncontrollable period for the textual critic is the fourth century B.C., can we dare to hope that better scribes than ours showed greater reverence for their author's words or be confident that corruptions which may have arisen at this time have been detected? Should we, for instance, in the light of our scribe's failure, ruthlessly restore a misplaced  $\tau\epsilon$  to its proper grammatical place? Secondly, the papyrus poses afresh the question of ancient editions. There is no ancient evidence as to whether the Alexandrians ever worked on the text of Thucydides. W. Schmidt<sup>25</sup> says they did not, V. Bartoletti<sup>26</sup> implies that they did not. B. Hemmerdinger<sup>27</sup> takes the division into eight books as both proof of and characteristic of an Alexandrian edition. Now the number of variant readings found in these scraps in less than eighty words of Greek contrasts strikingly with the much closer conformity to the manuscript tradition found in the papyri of Roman date. Is it likely that this conformity came about without deliberate editing? But if there was deliberate editing, who the editors were and on what principles they worked the papyri have not yet revealed.

#### Addendum

Through the kindness of Professor K. J. Dover I learn that  $\delta\iota\alpha\nu\omicron\iota\alpha$  for  $\pi\alpha\rho\alpha\sigma\kappa\epsilon\upsilon\eta$  (c.2.2) is also found in H = Paris gr. 1734. This agreement, inconceivable as due to mere coincidence in error (and consequently confirming B. Hemmerdinger's view, *Essai sur l'histoire du texte de Thucydide*, p. 55 f., that H was collated with an independent tradition), demonstrates that one ancient edition had  $\delta\iota\alpha\nu\omicron\iota\alpha$ , and makes it possible to argue that  $\delta\iota\alpha\nu\omicron\iota\alpha$ , as *lectio difficilior*, is the right reading.

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<sup>25</sup> In Schmidt-Stählin, *Gesch. d. gr. Lit.* I, 5, p. 200.

<sup>26</sup> *Per La Storia del Testo di Tucidide*, p. 67: 'Insomma, la tradizione tucididea fu tradizione spontanea, e quindi varia,

non trattenuta e incanalata per vie più ristrette da edizioni che in qualche modo s'imponessero nell'età successive'.

<sup>27</sup> *REG* 61 (1948), pp. 104 ff.





# PHRYNICHOS AND ASTYCHOS (THUCYDIDES VIII. 50-1)

AMONG the many intrigues described by Thucydides in Book viii one of the strangest and most obscure is the episode in which Phrynichos, while serving as one of the commanders of the Athenian fleet at Samos, twice sent messages to Astyochos, the Spartan ναύαρχος. Because the intrigue was conducted with the utmost secrecy, detailed information about it cannot have been easily obtainable, and the motives of all the persons involved, who included Alkibiades and Tissaphernes, were perhaps fully known to nobody. The account given by Thucydides (50-1) leaves much unexplained: it gives the impression that he has recorded what he has ascertained from a single informant without adding much comment or interpretation of his own. Had he lived to revise Book viii, he would scarcely have left these chapters as they stand. The object of this paper is to examine these chapters and to suggest that the intrigues of Phrynichos described in them were less exclusively personal in aim, and had somewhat more important consequences, than is generally believed.

The picture of Phrynichos drawn by Thucydides presents him from the outset as a man of exceptional shrewdness who held strong views and did not hesitate to press vigorously for their acceptance even where they were not shared by others. The arguments whereby he dissuaded his colleagues, immediately after their victorious land operations at Miletos, from risking a sea-battle against the newly reinforced Peloponnesian fleet are recorded in some detail (27. 1-4) and with explicit approval (27. 5). Shortly before the episode of his communications with Astyochos he opposed in outspoken terms the plan of the Athenian trierarchs and others who were negotiating with Alkibiades with the intention of overthrowing the democracy and obtaining Persian support. Thucydides devotes a long passage of *oratio obliqua* to the objections of Phrynichos (48. 4-7). He maintained that Alkibiades was indifferent to the proposed change of constitution and was interested only in securing his own recall; that to Persia the existing alliance with the Peloponnesians was more advantageous than an alliance with Athens could be; that the establishment of an oligarchy would not improve, and might well damage, Athenian relations with the allies. Thucydides expressly concurs with the first of these arguments (48. 4, ὅπερ καὶ ἦν), and his approval of the other two may perhaps be inferred from his subsequent narrative, which confirms their validity.<sup>1</sup>

It was at this point that Phrynichos dispatched the first of his secret messages to Astyochos. He informed him of the damage that Alkibiades was doing to Spartan interests by negotiating an agreement between Tissaphernes and the Athenians and gave a clear account of all the other machinations of Alkibiades. He defended this act of treason by maintaining that he was justified in seeking to damage an enemy even where in so doing he was harming the interests of his own country (50. 2). Astyochos then proceeded to Magnesia, where he communicated to Alkibiades and Tissaphernes the content of the message from Phrynichos.<sup>2</sup> Alkibiades at once reported to the Athenian leaders at Samos what Phrynichos had done and demanded his execution (50. 3-4). Phrynichos now sent a second dispatch to Astyochos remonstrating with him for his breach of confidence but offering to enable the Peloponnesians to destroy the entire Athenian force at Samos and giving details to show how this plan could be executed. He pleaded that this action, and indeed any other, was defensible where the alternative was death at the hands of his enemies. This message also Astyochos passed on to Alkibiades (50. 5), who sent a further communication to Samos in which he disclosed its content. Phrynichos somehow learned that this second letter from Alkibiades would soon arrive. Accordingly, he announced that the Peloponnesians planned to attack the Athenian base at Samos while the fortifications were incomplete, confidently declaring that his information was authentic. He then gave orders in his capacity as *strategos* for the speedy completion of the fortifications (51. 1). Hence when the second letter from Alkibiades arrived, its denunciation of Phrynichos was disbelieved: it was thought that Alkibiades was trying to use his knowledge of Peloponnesian plans to incriminate a personal enemy. Phrynichos thus extricated himself unharmed from the perilous situation in which his treason had involved him (51. 2-3).<sup>3</sup>

It is not unnatural that the veracity of this extraordinary and melodramatic story has been doubted in modern times. J. Hatzfeld suggests, after drawing attention to several features of the episode which evoke his suspicions, that the treasonable intrigue of Phrynichos with Astyochos was a fiction invented by Alkibiades in order to ruin his opponent.<sup>4</sup> This hypothesis, which its author

<sup>1</sup> A. G. Woodhead, *AJP* lxxv (1954), pp. 141-2, argues that the policy of Peisandros was preferable to that of Phrynichos. It must have been difficult even for contemporaries, if they were disinterested, to make up their minds on the question. The real issue was one that recurred at intervals towards the end of the fifth century (and for the last time in the *Frogs*), namely how far, and how long, to rely upon Alkibiades. It is at least arguable that Peisandros and Phrynichos may with equal sincerity have reached their diametrically opposed conclusions

on this problem. Thucydides does not state or imply that Phrynichos was at this stage influenced by personal antipathy towards Alkibiades.

<sup>2</sup> The report (ὡς ἐλέγτο) mentioned by Thucydides at this point that Astyochos was in the pay of Tissaphernes will be considered below (p. 102).

<sup>3</sup> The versions of this story by Plutarch (*Alkib.* 25. 6-13) and Polyainos (iii. 6) have no independent value.

<sup>4</sup> *Alcibiade* (1940), pp. 235-6.



does not develop,<sup>5</sup> is not at all convincing. It was inconvenient to Alkibiades that one of the *strategoi* at Samos should be trying to thwart his plan to secure his recall, but in view of the widespread support given by other leading Athenians this opposition can hardly have seemed so dangerous as to warrant the adoption of an elaborate stratagem designed to incriminate Phrynichos. Clearly the plan of Alkibiades was more embarrassing to Phrynichos than the opposition of Phrynichos was to Alkibiades. A more serious objection to the hypothesis of Hatzfeld is that the correspondence between Phrynichos and Astyochos cannot be separated from the rest of the story. The journey of Astyochos to Magnesia must have been known to many Peloponnesians, and the warning issued by Phrynichos of an impending attack on the Athenian base at Samos and his order to hasten the completion of the fortifications must have been known to large numbers of Athenians: they cannot have been invented by Alkibiades. While Astyochos might have visited Alkibiades and Tissaphernes for some purpose other than that of communicating to them the message from Phrynichos, and while Phrynichos might really have expected a Peloponnesian attack and taken measures to anticipate it, of which Alkibiades could have learned from his associates at Samos, it is only by an extraordinary coincidence that these events, which were outside the control of Alkibiades, could have occurred at precisely the moment when he might use them to provide the foundation of a false accusation against Phrynichos. There is no valid reason to doubt that the outline of the story as recorded by Thucydides, including the correspondence between Phrynichos and Astyochos, is authentic.

It will be convenient to examine the motives and aims of Phrynichos throughout this episode before discussing those of Astyochos. The motives attributed to Phrynichos by Thucydides are purely personal. He made his first approach to Astyochos because he felt sure that Alkibiades would be recalled and feared that Alkibiades, if he returned, would punish him for having expressed opposition to the plan for his recall (50. 1). It was also through fear that he sent his second message to Astyochos, since the letter from Alkibiades to the Athenian leaders at Samos disclosing his treason placed him in a highly dangerous position (50. 5).<sup>6</sup> There is, however, some reason to doubt whether Phrynichos acted wholly through fear for his own safety. Timidity can scarcely have been among the most prominent characteristics of this thrustful, independent upstart, incomplete though the picture of him drawn by Thucydides and others undoubtedly is. Even the comic poets apparently did not accuse him of cowardice as they accused Peisandros, and Thucydides warmly praises his constancy in perilous situations.<sup>7</sup> Phrynichos was probably himself responsible for the suggestion that purely personal motives caused him to communicate with Astyochos. It is most unlikely that Thucydides merely guessed these motives and much easier to believe that they were expressed in the text of the messages. Thucydides can hardly have seen the originals and doubtless depended for his knowledge of their content upon the report of some informant of which his account is a summary. The brief statements of motive which Thucydides prefixes to his version of both messages can scarcely have been ultimately derived from any source other than the original messages; indeed a phrase contained in his *oratio obliqua* summary of the second message (50. 5, ἡδὲ περὶ τῆς ψυχῆς δι' ἐκείνους κινδυνεύοντι)<sup>8</sup> may well have supplied him with the statement of motive with which he introduces this summary. If, as will be suggested below, the stratagem of Phrynichos was partly designed to influence the military and political situation in the interests of Athens, it was essential for him to conceal this aim from Astyochos, who must be persuaded that the information contained in both messages would lead to injury to Athens and benefit to Sparta.

It was undoubtedly the intention of Phrynichos that his first message would cause Astyochos to punish Alkibiades for his double dealing either by execution or by imprisonment, thus preventing his projected recall to Athens.<sup>9</sup> If this object had been attained, Phrynichos would at once have been relieved of all fears for his own safety, but a further consequence would have been that the whole elaborate scheme of Peisandros and others to secure Persian support by establishing an oligarchy and recalling Alkibiades would have been irrevocably ruined. Phrynichos had already advanced some potent political arguments against the adoption of this scheme (48. 4-7), and it is surely at least possible that one of his aims in communicating with Astyochos was patriotic, namely to bring to an end negotiations which in his view could not benefit Athens and would cause dissension at a time when unity must at all events be preserved.<sup>10</sup> If this suggestion be accepted, it removes a difficulty in the narrative of Thucydides. Phrynichos, who was very shrewd (27. 5), could surely have foreseen that his arguments against the scheme might fail to convince its promoters, who were already deeply committed (47. 2), and that in this event his outspoken opposition was likely to prove dangerous to himself. A little forethought could have kept him secure from any personal risk if he had considered only his own safety. On the other hand, his course of action

<sup>5</sup> Apparently he means that no messages were in fact sent by Phrynichos to Astyochos and not that the two messages were received by Astyochos, but had been forged by Alkibiades. The latter possibility can surely be dismissed because it was certainly not in the interests of Alkibiades that his schemes should become known to the Peloponnesians.

<sup>6</sup> τὸ μήνυμα is clearly that of Alkibiades (Steup, n. ad loc.).

<sup>7</sup> 68. 3, πολὺ τε πρὸς τὰ δεινὰ, ἐπιστήμην ὑπίστη, φερεγγυώτατος ἔραν.

<sup>8</sup> Steup, n. ad loc., is surely right in interpreting δι' ἐκείνους as the enemies of Phrynichos in the Athenian army at Samos.

<sup>9</sup> Cf. 50. 3, ὃ δὲ Ἀστυόχος τὸν μὲν Ἀλκιβιάδην . . . οὐδὲ διανοεῖτο τιμωρεῖσθαι.

<sup>10</sup> 48. 4, σφίσι δὲ περιόπτειν εἶναι τοῦτο μάλιστα, ὅπως μὴ στασιάζωσιν.



is much more intelligible, as well as being more in keeping with his constancy in perilous situations (68. 3), if he took the bold step of openly opposing the conspirators through concern for Athenian interests and partly at least for the same reason adopted the hazardous experiment of communicating with the enemy when he found that his opposition was ineffective.

Though this expedient was very ingenious, it was doomed to failure from the outset because it was based upon a misconception. Through lack of up-to-date information about the relations between Alkibiades and the Peloponnesians Phrynichos mistakenly assumed that he was still ostensibly co-operating with them, whereas he was already a fugitive at the court of Tissaphernes and therefore beyond the reach of Astyochos, who had received orders to put him to death (45. 1). That Phrynichos was mistaken about the position of Alkibiades is not expressly stated by Thucydides but may legitimately be inferred from his narrative.<sup>11</sup> If Phrynichos had known that Astyochos was no longer in a position to punish Alkibiades at will, he would scarcely have taken the risk of sending his message, which might have aggravated Peloponnesian relations with Tissaphernes but was not at all likely to hinder the negotiations between Alkibiades and the Athenian oligarchs. Even in a period when secret intelligence passed so freely from one side to the other, it is not surprising that Phrynichos acted in ignorance of developments at Miletos, which were very recent. Only a few weeks had passed since the flight of Alkibiades,<sup>12</sup> who does not appear to have informed the Athenian oligarchs that he was a fugitive<sup>13</sup> and probably wished them to imagine that he still enjoyed the confidence of the Peloponnesians.<sup>14</sup> It must have been widely known at Samos that he was at the court of Tissaphernes, but not that he had fled for his life and would not return to Miletos. The dispatch from Sparta ordering Astyochos to put him to death (45. 1) was probably disclosed only to a few Peloponnesian officers.

The decision of Phrynichos to send his second message to Astyochos, in which he offered to betray the Athenian base at Samos, would appear from the account of Thucydides to have been as imprudent as it was culpable. Unless he acted in a moment of panic without adequate consideration of the consequences, he can hardly have failed to appreciate that Astyochos might well report the second message, as he had reported the first, to Alkibiades, who would certainly inform the Athenian oligarchs.<sup>15</sup> The difficulty of believing that Phrynichos did not foresee this danger has led scholars to conclude that he deliberately planned the whole sequence of events that ensued after the dispatch of his second message; that he had no intention of implementing this offer to betray the Athenian base and sent the second message in order to extricate himself from his perils, expecting that it would be passed by Astyochos to Alkibiades and by Alkibiades to the Athenians, to whom Phrynichos would meanwhile have issued his warning to expect a Peloponnesian attack.<sup>16</sup> This conclusion is surely inescapable, even though Thucydides gives no hint that the offer made to Astyochos was insincere and indeed implies that Phrynichos decided to warn the Athenians only after he learned that Alkibiades had been informed of this offer and was reporting it to Samos.<sup>17</sup>

If this explanation be accepted, Phrynichos calculated that Astyochos would treat the second message as he had treated the first. Clearly, however, he could not be absolutely certain that this forecast would prove correct. Astyochos had achieved so little success that the Spartans sent out eleven commissioners to advise him and, if they thought fit, to relieve him of his command,<sup>18</sup> and the prospect of destroying the Athenian forces at Samos and thus probably being instrumental in bringing the war to a speedy end was a dazzling one that can hardly have failed to attract him. The possibility that, instead of communicating to Alkibiades the offer of Phrynichos, he might seek to seize the opportunity that it seemed to present could not be entirely ignored. Phrynichos must surely have foreseen this possibility but have believed that military and political benefits for the Athenians, as well as profit for himself, would result from a Peloponnesian offensive. The detailed

<sup>11</sup> Especially 50. 3, ὅλλως τε καὶ οὐκ ἐστὶ ὁμοίως ἐς χεῖρας ἵκνται.

<sup>12</sup> The chronology of this winter (412-11) is obscure, but it seems probable that the flight of Alkibiades took place in November and the intrigues of Phrynichos, which must have occupied two or three weeks, in December (cf. W. S. Ferguson, *CAH* v. (1927), pp. 319 and 323).

<sup>13</sup> There is no reference to his flight in 47. 2-49, where his first contacts with them are described.

<sup>14</sup> Cf. 51. 3, from which it is clear that the Athenians at Samos, when they received the message from Alkibiades disclosing the offer of Phrynichos to betray their base, assumed that the former was still receiving full information about Peloponnesian plans.

<sup>15</sup> Cf. Hatzfeld, *op. cit.*, p. 235. Another objection raised by Hatzfeld, *op. cit.* p. 236 n. 1, namely that Phrynichos was not impeached for treason after his return to Athens, is invalid. Peisandros secured the dismissal of Phrynichos from his command by accusing him of having betrayed Iasos (54. 3). Thucydides states that this charge was false, and it seems to have rested only on the fact that the Athenian withdrawal from Miletos, for which Phrynichos was responsible, left Amorges to defend Iasos unaided (28. 2-4). The charge was evidently dropped, and Phrynichos soon afterwards reappears as one of

the leading oligarchs (68. 3, cf. Arist., *Pol.* v. 1305 b). Peisandros could not have accused him of having communicated with Astyochos without revealing his own intrigues with Alkibiades, who was a public enemy (cf. Woodhead, *op. cit.*, p. 142).

<sup>16</sup> Grote, *History of Greece* viii. 3 (1855) p. 17, seems to have been the originator of this explanation, cf. T. Lenschau, *RE* xx (1941) col. 908, and P. A. Brunt *REG* lxx (1952), p. 76.

<sup>17</sup> It is tempting to accept the explanation of ὡς προήσθετο αὐτὸν ὁ Φρύνιχος ἀδικούντα in 51. 1 given by Steup, n. *ad loc.*: 'nach der gemachten Erfahrung sah Phryn. voraus, dass Astyochos abermals Verrat üben werde.' The words, however, surely mean 'when Phrynichos learned in advance' (i.e. in time to take the action described in the main clause, cf. 16. 2 and 79. 3 where προαισθάνομαι is similarly used) 'that he was doing him an injury' (i.e. the injury mentioned in the preceding sentence, ὁ δὲ Ἀστυόχος μὲνεται καὶ ταῦτα τῷ Ἀλκιβιάδῃ). The second participle dependent upon προήσθετο, namely ὅσον οὐ παροῦσαν ἀπὸ τοῦ Ἀλκιβιάδου ἐπιστολήν, shows that this interpretation is correct.

<sup>18</sup> 39. 2. He was also perhaps already unpopular with his own troops, whose insubordination later almost cost him his life (78; 83. 3-84. 3).



information given to Astyochos showing him how best to attack the Athenian base (50. 5) was doubtless such that if the Peloponnesians tried to make use of it the Athenians would enjoy an initial advantage in the ensuing engagement. Hitherto Astyochos had shown a marked reluctance, which was probably justified, to take offensive action: if he could be tempted to deliver an attack of which the Athenians would be forewarned, the consequences were likely to be disastrous for the Peloponnesians.<sup>19</sup> A Peloponnesian attack would stiffen Athenian morale after a period of little activity, and a military victory might well have political consequences in causing the abandonment of the plan to obtain Persian support after overthrowing the democracy and recalling Alkibiades. It would at the same time restore the credit of Phrynichos, who would probably no longer have anything to fear from the oligarchs negotiating with Alkibiades. A further possibility which Phrynichos must have considered was that Astyochos might suspect that the second message was an attempt to beguile him into taking ill-advised action and might, without informing Alkibiades, merely ignore it. This possibility was probably less likely than the other two. Phrynichos doubtless calculated that, although in this event he would not be so likely to escape the consequences of having sent his first message, the warning of an impending attack by the Peloponnesians which he intended to issue would hasten the completion of the fortifications at Samos. This work, which was desirable on military grounds, was in fact completed sooner than it would otherwise have been (51. 2), and in the atmosphere of disunity created by the intrigue of the oligarchs it was perhaps only by a warning of this kind that even a *strategos* could secure the prompt execution of his orders. All these considerations suggest that the offer of Phrynichos to Astyochos was not designed exclusively to extricate him from a dangerous situation but also to promote what he considered to be the best interests of Athens.

The motives and aims of Astyochos in this episode seem to have been almost wholly unknown to Thucydides. His journey from Miletos to Magnesia cannot have been undertaken solely to enable him to inform Alkibiades and Tissaphernes of the first message from Phrynichos: a mere report could have been delivered by a messenger or a subordinate officer. He surely made the journey because he considered negotiations of some kind to be necessary which could be conducted only by himself. He may have wished to discuss the vexed question of Persian pay for the crews of his ships,<sup>20</sup> but it is much more likely that his purpose was to remonstrate with Alkibiades and Tissaphernes about the intrigues with the Athenian oligarchs reported to him by Phrynichos and to attempt to deter Tissaphernes from concluding an agreement with the Athenians. What took place at this important interview is not recorded by Thucydides, who states only that Astyochos divulged the content of the message received from Samos and was rumoured (*ὡς ἐλέγετο*) to have succumbed to bribes from Tissaphernes (50. 3). This charge was almost certainly false;<sup>21</sup> it seems to have originated only from subsequent gossip on the part of his disgruntled troops,<sup>22</sup> when his failure to secure pay for them led to mutinous disturbances (84. 1-3).

While Astyochos seems to have discharged his duties loyally and conscientiously in difficult conditions, he was certainly not a man of very high ability. His caution was largely justifiable, but he showed a lack of initiative and imagination which recalls that of his predecessors in the Archidamian war, Knemos and Alkidas. He also seems to have combined weakness with quarrelsomeness and to have lacked diplomatic finesse, so that he often failed to secure the willing co-operation of others. Such is the impression created by the narrative of Thucydides, who, when he creates impressions of this kind, does so by design and not by accident.<sup>23</sup> When Astyochos visited Alkibiades and Tissaphernes at Magnesia, he must have been at a disadvantage in having to deal with two crafty and experienced diplomatists, and it may be that his inability to hold his own on this occasion appeared to confirm the subsequent charge of having accepted bribes from Tissaphernes. He was not even in a strong bargaining position. He knew that if Tissaphernes were to negotiate an agreement with the Athenians, the Peloponnesian fleet would be in an almost desperate position; he could perhaps only point out that such an agreement would constitute a violation of the recently concluded second treaty between the Peloponnesians and Persia (37. 4) and that he might not be able to prevent his forces from plundering Persian territory if they remained long unpaid.<sup>24</sup> Tissaphernes doubtless denied that there was any truth in the allegations of Phrynichos. He could do so

<sup>19</sup> It was also desirable that this attack should take place before the arrival of a substantial reinforcement which, as the Athenians may have learned, was shortly to sail from the Peloponnese (39. 1).

<sup>20</sup> It is not clear from the somewhat confused narrative of Thucydides whether he played any part in the conclusion of the second treaty between the Peloponnesians and Persia (36. 1-2) unofficially known as the treaty of Therimenes (52).

<sup>21</sup> The most elaborate defence is that of G. Fabrizio, *Contributo storiografico-storico allo studio della guerra decelica* (1946), pp. 5-17, who concludes, rightly in my opinion, that the refusal of Astyochos to engage the enemy on three occasions was justified on military grounds and was not the outcome of bribery by Tissaphernes.

<sup>22</sup> 83. 3, cf. 78. Fabrizio, *op. cit.*, pp. 12-13, maintains that the words *καὶ γίνετο το αὐθιγότερο* in 50. 3 are a later insertion

in the account of this episode, being added by Thucydides after he had written 83. 3, which was derived from a different source.

<sup>23</sup> Examples include his treatment of Nikias (as I have attempted to show in *CQ* xxxv (1941), pp. 58-65) and of Agis (in v).

<sup>24</sup> Cf. the fears of Tissaphernes mentioned in 57. 1. I cannot agree with the contention of Fabrizio, *op. cit.*, pp. 13-14, that Astyochos 'si trovava con quella lettera in mano in una situazione diplomatica fortissima'. It is true that the message from Phrynichos supplied evidence of disunity at the Athenian headquarters at Samos, but Phrynichos must have stated that he expected the oligarchical plot to succeed if Alkibiades were not removed, and that its success would lead to the conclusion of an agreement with Tissaphernes.



with some degree of sincerity; for, though evidently attracted by the proposals of Alkibiades (46. 5, cf. 52), he does not seem to have ever passed beyond the stage of weighing the possible advantages and disadvantages of coming to terms with the Athenians. Alkibiades must have denied, no less emphatically but much less honestly, that he was intriguing with Athenian leaders at Samos. He could with some plausibility argue that the message from Phrynichos was a calumny designed to ruin a dangerous enemy, and he probably ridiculed the suggestion that he wished to be recalled to Athens or that the Athenians wished to recall him. Astyochos is believed, on somewhat slight grounds, to have belonged to the same party as Endios, the friend and collaborator of Alkibiades,<sup>25</sup> and may on that account have been more inclined to accept the assurances which he now received. At all events, he seems to have satisfied himself that Alkibiades and Tissaphernes were not betraying Peloponnesian interests.

The courses open to Astyochos on receiving the second message from Phrynichos have already been discussed. It was doubtless Spartan caution and distrust that led him to report its contents to Alkibiades, as Phrynichos apparently expected. He seems to have been still at Magnesia when the second message reached him, and he probably decided, because he trusted Alkibiades more than he trusted Phrynichos, that the most prudent course would be to consult the former on the feasibility of the plan to attack Samos which Phrynichos had proposed. It is true that Phrynichos in his second message reproved Astyochos for having failed to keep the first message secret (50. 5), but if Astyochos was convinced that Alkibiades still favoured the Peloponnesian cause and was not intriguing to win the support of Tissaphernes for Athens, he can have seen no harm in seeking his advice. He doubtless believed that the action of Alkibiades in disclosing to the Athenians at Samos the content of the first message was a personal reprisal against Phrynichos, but that Alkibiades would take a very different view of the second message because it seemed to offer to the Peloponnesians the prospect of a decisive victory.

This episode appears at first glance to have been of very little importance and to have had scarcely any influence upon the development of the complicated situation in Ionia. Because Phrynichos was unaware that Alkibiades was beyond the reach of the Peloponnesians and could not be punished for plotting to win the support of Tissaphernes for the Athenians, the first message to Astyochos failed to wreck this plot; through the clever expedient of his second message Phrynichos succeeded in escaping from the dangerous situation into which his first message had brought him. The only significant result of the episode appears to have been to hasten the completion of the fortifications at Samos, but though the effort demanded by this work and by other preparations for defence may temporarily have improved Athenian discipline, its effect can scarcely have lasted long when the expected attack did not take place. There is, however, some reason to believe that the stratagem of Phrynichos did indirectly produce important results.

Tissaphernes eventually refused to allow himself to be persuaded to transfer his support to the Athenians (56. 3) and then proceeded to conclude his third treaty with the Peloponnesians (57). It may be that he would not in any event have consented to commit Persia to an agreement with the Athenians,<sup>26</sup> but, as has been already mentioned, he was certainly attracted by the suggestions of Alkibiades, who would hardly have proceeded so far in his negotiations with the Athenian oligarchs if he had considered his attempt to win Tissaphernes to be doomed to failure from the outset. The reasons given by Thucydides for the final decision of Tissaphernes are complex and somewhat confusingly stated,<sup>27</sup> and it is not relevant to discuss them here. It is, however, almost certain that the revelations of Astyochos in the conference at Magnesia were embarrassing to Alkibiades in that they upset the course of his carefully laid scheme to lead Tissaphernes gradually forward from the negative policy of withdrawing support from the Peloponnesians to the positive policy of giving support to the Athenians. The message from Phrynichos disclosed that Alkibiades had at least offered the oligarchs hopes of winning Tissaphernes for Athens, though he had perhaps given no definite undertaking.<sup>28</sup> On the other hand, Alkibiades in his consultations with Tissaphernes had apparently so far merely argued on general grounds that the Persians would find the Athenians more convenient allies than the Peloponnesians (46. 3); it was only after the conference at Magnesia that he openly urged Tissaphernes to conclude an agreement with the Athenians (52).<sup>29</sup> To Tissaphernes the discovery of a discrepancy between what Alkibiades had told the Athenian oligarchs and what he had told him must have been very disquieting. If he thought that he had secured the services of an expert in Greek methods of diplomacy, he must now have realised, perhaps for the first time, that Alkibiades was seeking to promote his own interests and those of Athens rather than those of Persia. Alkibiades remained for some time at the court of Tissaphernes, who continued to follow

<sup>25</sup> G. Busolt, *Gr. Gesch.* iii. 2, p. 1469. This view is apparently based on the fact that Endios was ephor in the same year (412-11) in which Astyochos was ναύαρχος. The further suggestion (Busolt, *loc. cit.* and p. 1437 n. 6, cf. Grote, *op. cit.*, viii<sup>2</sup>, p. 3) that Astyochos warned Alkibiades of the order for his death is much more questionable, though possibly Astyochos deferred action until he should himself have reached Miletos and could investigate the complaints against Alkibiades.

<sup>26</sup> Cf. the argument of Phrynichos in 48. 4.

<sup>27</sup> The contention of Wilamowitz, *Hermes* xliii (1908), pp. 594-5, that there is an inconsistency between Chapters 56 and 57, in which these reasons are stated, is rightly rejected by Brunt, *op. cit.*, pp. 87-8.

<sup>28</sup> 50. 2, cf. 48. 1, ὑποσχείνοντος αὐτοῦ Τισσαφέρνην μὲν πρῶτον, ἔπειτα δὲ καὶ βασιλέα φίλον ποιήσιν.

<sup>29</sup> Brunt, *op. cit.*, p. 86 (though the argument of Alkibiades in 46. 3 seems to me to amount to more than a hint).



his original recommendation to maintain a balance between Peloponnesians and Athenians, allowing them to exhaust one another (46. 1 and 4; 56. 2; 57. 2). Yet the feelings of uneasiness engendered in the mind of Tissaphernes by the conference with Astyochos are likely to have had a strong, perhaps even decisive, influence upon his ultimate rejection of the projected agreement with the Athenians. Hence the stratagem of Phrynichos, while it failed to ruin Alkibiades, may well have eventually proved fatal to his plan,<sup>30</sup> which, had it succeeded in effecting even a temporary agreement with Tissaphernes, might have changed the course of history.<sup>31</sup>

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<sup>30</sup> It is significant that the posthumous condemnation of Phrynichos was for treasonable conduct while in command at Samos (*F Gr Hist* 342 F 17) and not, as might have been expected, for any of his activities as a leader of the Four Hundred, such as his participation in the embassy to Sparta (90. 2, *cf.* [Plut.], *Mor.* 833 e-f). The proposer of the decree was Kritias (Lykourg. 113), who was apparently at the time an agent of

Alkibiades at Athens (Plut., *Alkib.* 33. 1). Alkibiades thus seems to have instigated this prosecution of an enemy whose action had indirectly frustrated one of his most promising schemes.

<sup>31</sup> I am much indebted to Professor A. W. Gomme, whose criticisms of this paper have been of the greatest value to me.

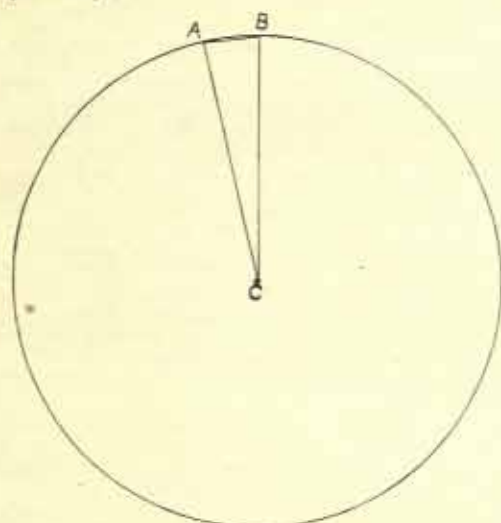


## NOTES

### Thales and the Diameter of the Sun and Moon.

In Vol. LXXV (1955) of the *JHS* I suggested that the determination of the angular diameters of the sun and the moon ascribed to Thales (Diog. Laertius I. 24) may have been obtained by angular measurement, not as is generally supposed by time-measurement. However, the question of the precise technical method that may have been employed was left open. To measure a very small angle with any degree of accuracy is obviously not easy; and a combination of actual measurement with calculation is probably necessary. In what follows I describe a method of measuring very small angles: whether this was the method employed in obtaining the result ascribed to Thales I do not know; all I can claim is that it presupposes neither mathematical knowledge nor mathematical techniques which could not have been at the disposal of an early Greek philosopher-mathematician.

Given the following assumptions (and granted that they could be known to a Greek) the diagram set out below is almost self-explanatory:



(a) In the case of a very small angle the length of the arc and the chord subtending that angle are nearly equal, and any difference in their length can, for practical purposes, be disregarded.

(b) The circle is divided into  $360^\circ$ .

(c) The ratio of the length of the circle to the diameter is constant. What this ratio  $\Pi$  is taken to be is not very important. Some approximations to the true value were made in early antiquity; and we are told that some Babylonians determined the value  $\Pi = 3$ .<sup>1</sup> In what follows we shall use  $\Pi = 3$ ; but I must emphasise that any other value for  $\Pi$ , more or less accurate, would do as well for our purposes. I have chosen  $\Pi = 3$  simply because this value may have been known to Thales; and also because it happens to make the following calculations easier.

Let  $\angle ACB$  be the angle to be measured (the size of the angle in the diagram is, for purposes of illustration, much enlarged).

Then the chord  $AB$  is for practical purposes equal to the arc  $AB$ . Therefore the following equation holds good

$$\frac{AB}{2\Pi AC} = \frac{\angle ACB}{360^\circ}$$

The length  $AB$  can be measured; and  $2\Pi AC$  is a constant, or can be made a constant. Thus the equation with  $\angle ACB$  as the unknown can easily be solved. However, the result might be any rational fraction; and it would be an almost unbelievable coincidence if the result were not only fairly accurate, but also elegant, namely,  $\frac{1}{18}$  of the circle, i.e.,  $30'$  of angle.

We therefore go one step farther and ask ourselves: supposing that  $\Pi$  is taken to be  $= 3$ ,<sup>2</sup> what constant distance  $AC$  will give us a formula such that we can dispense with any further calculation and simply regard the value of the unknown  $\angle ACB$  as directly given by the measurement of the distance  $AB$ ? The question need only be asked for the answer to be immediately obvious, for any value  $AC$  such that  $AC$  multiplied by  $2\Pi = 360$  will fulfil our requirements; and if  $\Pi$  is taken to be  $= 3$ , then we take  $AC = 60$ . It does not matter what unit of measurement we use; it can be yards, inches, metres, centimetres, or any other unit of measurement, or any combination of units, or any multiple or fraction of such a unit. What is important is this: that once we take the distance  $AC$  (which, of course, we can choose and fix arbitrarily) as 60 units of length, our first formula (taking  $\Pi = 3$ ) becomes

$$\frac{AB}{360} = \frac{\angle ACB}{360^\circ}$$

And therefore the measurement of  $AB$  in terms of the unit employed for fixing  $AC$  will give us the immediate solution for  $\angle ACB$ . Let  $AC$  be 60 feet, or inches, or yards, or any other measure; then measure  $AB$  by the same unit of length and you will have the answer in degrees of angle for the  $\angle ACB$ . Suppose the length of  $AB$  1 foot (or inch, or yard), then the angle will be 1 degree. Suppose the length of  $AB$  is  $\frac{1}{2}$  a unit of length, then the angle is  $30'$ ; and so on.

It is plain that this method is applicable only to very small angles and that it will not lead to very accurate results. But what is interesting is this: (a) the result will always be expressed in degrees of angle (i.e.,  $\frac{1}{360}$  of a circle), and (b) the result will be more or less elegant in inverse proportion to its accuracy. That is to say, a result of  $30'$  for the diameter of the sun obtained by an early mathematician is not unbelievable, for its very elegance is the outcome of a very low degree of necessary accuracy of measurement. All that is required is that half of one unit of measurement be measured with no more than rough approximation.

It will also be observed that no circle need actually be drawn. All that needs to be done is to pace out any convenient distance  $CA$  or  $CB$ ,  $A$  and  $B$  being in the direction of the limbs of the sun (or of any other distant object the angular diameter of which is to be measured). If this distance is such that  $AB$  can be conveniently measured in the same unit of measurement, no more is needed. In fact, if the unit of measurement initially employed is itself such that it can conveniently be subdivided into smaller units of 60 each, then we can (at least in theory) use this method for directly measuring angles in terms of minutes, not only degrees.

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### The Duration of the Samian Tyranny.

In *JHS* LXXIV 36-43, under the above heading, M. White argues from Hdt. III 47-9 that the offences cited as provoking the Sparto-Korinthian expedition against Samos ought to have been committed either by Polykrates himself or by a predecessor of similar policy 'whose sins could justly be visited upon his head'. These offences were: (1) the interception of the Kerkyraian boys sent by Periandros to Alyattes; (2) the seizure of the corslet sent by Amasis to Sparta; and (3) the seizure of the bowl sent by the Spartans to Kroisos. Of the boys, Miss White says (p. 37) that they must have been sent 'at latest before the death of Alyattes ca. 560-555 B.C.'. The corslet was seized a year before the bowl, which 'was a gift on the occasion of the alliance made between Sparta and Kroisos

<sup>1</sup> See M. Cantor, *Vorlesungen über Geschichte der Mathematik*, I, 91 (see also p. 93). He argues that the Babylonians used the value  $\Pi = 3$ . This was probably only a rough and ready measure useful for practical purposes; and as such it was probably in use all over the Middle East. Thus,  $\Pi = 3$  is implied in the measurements given in the Bible, viz. *I. Kings*, 7. 23 and *II. Chronicles*, 4. 2. The same value is again implied in the much later Babylonian Talmud, for which see Cantor, *Zeitschrift Math. Phys. XX* histor. literar. Abteil. 164.

<sup>2</sup> This argument could, of course, be adapted to the supposition of any other value for  $\Pi$ .



shortly before the latter's fall'. As to the date of this event, Miss White is not prepared to adjudicate as between the various years proposed, from 547 to 541/0: 'even at the latest it is well before the time of Polykrates' (n. 10). Eusebios' date for the accession of Polykrates is c. 532; and, even if this tradition is set aside, a date much earlier than 540 is impossible, because Peisistratos' establishment of Lygdamis as tyrant of Naxos, some time after his own final establishment at Athens (547/6), must come first. Therefore Polykrates himself cannot have been responsible for the seizure of either the corslet or the bowl—much less the interception of the boys. Miss White suggests that these and other considerations favour the view that the tyranny began in the sixties, and that Aiakes, Polykrates' father, was the first tyrant.

That Hdt. III 47 implies a date earlier than c. 532 for the commencement of the Samian tyranny I have long held and taught; and Miss White's view that it may have begun with Aiakes as early as c. 560 I find persuasive on many counts. But I cannot agree with her use of the incident of the Kerkyraian boys or with her rejection of the view that Polykrates himself may have been responsible for the seizure of the corslet and the bowl.

In III 47-9 Hdt. is uncritically repeating what he has been told, without regard to logic, chronology, or general historical probability. He sets forth the Samian and Spartan versions of the motives for the expedition without noticing—or at least without making explicit in his usual manner—that the two versions imply two categories of 'Samians'—those whose services are to be requited and those whose piracy is to be chastised. He puts the interception of the boys at about the same time as the seizure of the bowl, though he knows well enough that the boys were sent to Alyattes and the bowl to Kroisos, at the end of his reign. And he fails to realise that, unlike the motives ascribed to the Spartans, which may indeed, along with others, have entered into their deliberations, that ascribed to the Corinthians is in itself highly improbable. However exacerbated by their continued differences with the Kerkyraians, Corinthians of c. 525 can have wished only to forget that Periandros had barbarously destined 300 innocent

Kerkyraian boys for emasculation at the court of Alyattes; and they must, if reminded, have been willing to admit that the Samians had in fact rendered both Korinth and Greece a service by intercepting them. Whoever suggested to Hdt. that this was a Korinthian motive (and he does not tell us who did) must have been wrong; and accordingly we cannot assume that the interception was the work of a Samian tyrant.

There is in any case a grave oversight in Miss White's chronology. The boys were sent by Periandros, who (as all scholars save Beloch have agreed) died c. 587. This, then, is the *terminus ante quem* for the affair, and not the death of Alyattes a generation later. Whether Miss White would be prepared to stretch the reign of Aiakes to a duration of about sixty years, or to suppose that he was not the first tyrant, I do not know.

As regards the corslet and the bowl, unless we are prepared to stand by the Eusebian date for the accession of Polykrates (and I do not think it receives as much support from Hdt. and Thuc. as Miss White suggests), there is no chronological difficulty in supposing that he may have been responsible for the seizure of both. Miss White rightly dates the battle of Pallene thirty-six years before the expulsion of Hippias (though I should prefer 546/5 to 547/6), and admits that the fall of Sardis may have been as late as 541/0. This is surely on other grounds the most probable year for the event. Between 546/5 and 541/0 there is ample time for the establishment of Lygdamis at Naxos, the subsequent establishment of Polykrates at Samos, and the seizure of the corslet, one year before the seizure of the bowl, which was contemporary with the fall of Sardis. (The gift was indeed occasioned by the alliance, which was a little earlier, but the work will have taken some time to execute and despatch, and the Samian excuse, that it arrived too late to be given to Kroisos before the fall of Sardis, must at least have been plausible.)

In so far then as she is relying upon Hdt. III 47-9, I cannot feel that Miss White has proved that Polykrates must have had a predecessor: this is not to say that there is not considerable weight in some of her other arguments.

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## NOTICES OF BOOKS

**Fifty Years of Classical Scholarship.** Ed. M. PLATNAUER. Pp. xiii + 431, with 4 plates. Oxford: Blackwell, 1954. 31s. 6d.

This book is of value principally for the four or five first-rate pieces of writing that it contains, though it will prove useful to scholars who want an up-to-date record of recent work in the subjects with which it deals. Only very well-informed amateurs are likely to interest themselves in the sort of exposition presented by most of the contributors or to find the bibliographies to their purpose. The scope and scale of the work suggest a good many queries, not all of which answer themselves. The interpretation of the term Classical Studies is somewhat arbitrary. We have chapters on literature, on philosophy though not on religion or science, on the historical writers but not on history, and nothing on archaeology or philology except in connection with Homer. Even in literature there are curious gaps: Hesiod is absent though he has been the object of not a little learned speculation in the last fifty years; Ennius appears only as a dramatist; the *Poetics* receive no more than casual mention, though the modern tendency to remould the theory of *catharsis* nearer to the heart's desire could have been made the subject of some interesting reflections. Nor does the period from 1904 to the present day possess much unity as an epoch in the history of scholarship. Indeed, many contributors virtually ignore it as a limit. But already the book is long and not inexpensive, and it would perhaps be impracticable to produce a book which really did justice to the title. Since it is unlikely much to affect the notion of the usefulness and vitality of classical studies entertained by the Great Paymaster, it matters less that the most dramatic chapter, that on archaeology, remains unwritten.

The chapters are of very unequal interest. This is partly due to their subjects. In some cases there are well-marked developments to be recorded and important tendencies to be remarked, in others progress has been piecemeal and no particular trend is visible; references to largely unrelated scraps of papyrus inevitably suggest a catalogue, though in view of the diversity of publications such a catalogue may be useful. But the quality of each chapter turns largely on the ability of the writer to decide what his object is and to keep that object firmly in mind throughout. 'The advance made along the main lines of classical scholarship during the period' is, the Editor asserts, the theme of the book. But although this advance has been made mainly by the publication of books and articles, a list of such things does not of itself constitute the story of the advance. *Fifty Year's Works* compressed into a single chapter do not tell us what we want to know about the work of fifty years. What is the difference between the state of knowledge then and now? What have we got right that our predecessors got wrong? In what ways are we better equipped to tackle a problem today than in the past? These are the questions to which we want the answer, and the success of each contributor bears some relation to his ability to keep these questions in the forefront of his mind. They cannot be answered without judgements of value franker than some contributors are prepared to make. Nor can much be achieved by anyone whose main object is not to leave unmentioned any work which might be thought by someone to deserve mention. Bibliographies are not inaccessible to the sort of reader who may be expected to use this book; those given here ought to be firmly subordinated to the main purpose of each article, which is to show the trend of development in a particular subject.

It would require a team of reviewers to criticise adequately and in detail the work of the distinguished band of scholars here assembled. The individual, once he leaves the ground familiar to him, is likely to reveal his own limitations rather than theirs. What follows must be for the most part a general impression.

Sir David Ross, writing on the Greek Philosophers, comes nearest to giving an undisguised book-list, to which, incidentally, should be added F. Wehrli's *Die Schule von Aristoteles*. His own interest, as he admits, is mainly in Plato and Aristotle, and he omits altogether Antiochus, Albinus, Plutarch, and Marcus Aurelius. This is the more to be regretted, since he shows in a paragraph on the Socratic controversy and in the few general remarks that he allows himself at the end how much more than a list he could have produced. No field of study covered by this book gives more scope for a review of changes in intellectual climate and of the interplay between ancient and modern.

Another subject which offers great opportunities is Homer and the Homeric question, which, though the longest discussed, is still one of the most alive. Archaeology and philology, here well represented by Miss Gray and Professor Palmer, continue to supply fresh evidence, and the comparative study of poetry associated with the names of Chadwick and Milman Parry has given the whole problem a new look. Professor Dodds, a practised expositor of this subject, gives a candid and lucid account which is one of the best things in the book. He will bring small comfort to strict unitarians, but in his interesting glance ahead at the discussions now beginning about the implications of the transfer of oral poetry to papyrus he perhaps underestimates the amount of ground which the unitarians by a few judicious concessions may hope to recover.

In contrast to the study of epic, that of Aristophanes owes little to new sources of knowledge, and of Old Comedy in general not much. Accordingly, the vigour and vitality of Professor Dover's chapter is the more remarkable, though he falls a victim to his own exuberance when he writes of Cornford's 'dishonesty'. He keeps the essential question in mind and has no doubt of the superiority of modern scholarship in this field and of the reason for it. It is due to the realisation that Greek was 'a real noise that once issued from the mouths of real men' and that Old Comedy was drama, though drama which by concentrating on the comic moment involved itself in such gigantic inconsequences that the modern imagination accepts them only with difficulty. As between Professor Dover on the one hand, and Kock or Van Leeuwen on the other, this may be fair enough, but it is worth remembering that these truths were not concealed from the 'amateur' Rogers, and that Frere could write of 'the utter extravagance and impossibility of the action . . . a grave, humorous, impossible, great lie', and set a familiarity with Foote's farces against Prof. Dover's acquaintance with *Take it from Here*. Amid much that is wisely said and tellingly put one may question his inclination to play down the literary expertise of the Athenian audience. A large proportion of the audience must have been in their time members of a chorus, and anyone who visits a village Eisteddfod realises that technical knowledge can be widely diffused in a community where genuine popular art still flourishes. The account of the relationship of Old, Middle, and New Comedy with each other and with tragedy is marked by striking good sense. The suggestion that it was Menander's practice 'to open the play with a dramatic scene . . . before entrusting the clarification of the situation to the speaker of the prologue' might be brought into relation with the practice of later tragedy as exemplified by the *Rhesus* and *Iph. Aul.* if, as is now more commonly supposed, the beginning of the latter is to be accepted more or less as it stands. In the use of evidence from vase-paintings he is distinctly wary.

Another contribution which shows a striking mastery, the more so since the developments recorded are 'on unspectacular and evolutionary lines', is that of Mr. Griffith on Greek Historians. He writes with refreshing frankness on the scandal of the Oxyrhynchus Historian whose continued elusiveness, whether he is a known or an unknown, can hardly fail to cast a dubious light on one of the principal activities of ancient historians. The chief novelty among the ideas of the period, the 'common peace', he treats with the faintest hint of scepticism. The problem of the composition of the History of Thucydides, and still more that of Herodotus, remains essentially open, and with regard to the latter the confusion is, if anything, increased now that Gomme has come down on the side of Macan. Dr. McDonald, in a somewhat drier manner, surveys Roman historians from the early annalists to Marcellinus; his story, too, is of piece-meal progress rather than of revolutionary conceptions.

Prof. Webster, finding his subject too large to discuss in relation to its historical development, has taken as his starting point Lesky's articles in the post-war *Anzeiger für die Altertumswissenschaft* which link up with Bursian and confined himself practically to an account of the present state of knowledge in the many departments of the scholarship of tragedy. Even so, his space is none too extensive for his matter. But it is impossible not to regret the account which Webster could have given of the phases and fashions which the study of tragedy has passed through in the last half-century, and also the paradoxical consequence that the chapter on tragedy mentions Verrall only incidentally and Headlam not at all. That we have a completer appreciation of tragedy than our predecessors and a clearer



notion of what the tragic poets were trying to do is probably true, but Webster is inclined to be optimistic as to the extent of the gain in positive knowledge. Valuable as recent work on satyrs and sileni may have been, there is still no agreement as to what a satyr was when he was not a silenus. No one can be accused of mere perversity if he is unconvinced of the connection of satyrs in general and the choreutae on the Pronomos vase in particular with goats, or of the presence of a goat-chorus in the *Cyclops* and *Ichneutae*. Again, in matters of theatrical production we may feel some confidence that a number of beliefs held fifty years ago were wrong, but it is unsafe to assert as a fact that an *ἐκκυκλῆμα*, in any ordinary sense of the word, was standard in the fifth century. 'Wir danken es E. Bethe, dass er die Bühne des 5. Jahrhunderts von dem Gespenst des Ekkyklima befreit hat', says Lesky, and Pickard-Cambridge was clearly more than half inclined to accept these the second thoughts of Bethe. With regard to literary criticism more attention might have been given to the marked break with older methods to be seen in such a book as Goheen's on the *Antigone*. It has long been made a reproach against classical scholars that they are supremely indifferent to the analytical style of criticism which is in fashion in other quarters; the method is less unfamiliar as applied to the Latin poets, especially Virgil, and, to judge from Mr. Levens's account, Ilse Schnell's investigations of Catullus were of a like nature. It is likely that, for good or ill, the number of such studies will increase in the future. These, however, are points of detail. Webster has made an able survey of a very wide field.

Mr. Barber, in his account of Hellenistic poetry, gives a useful description of the many new papyrus fragments which have come to light. The effect has been to widen rather than to deepen our knowledge of the Hellenistic world, and there is no fundamental change in interpretation to record. With regard to the great Alexandrian quarrel Barber seems unwilling to go the whole way with Ziegler and to consider Callimachus rather than Apollonius as the heretic. It is noticeable that the *Aitia* and the *Coma Berenices* are mentioned throughout as separate works; it is not clear whether this indicates doubt of the belief of Pfeiffer and others that the *Loek* was part of a later issue of the *Aitia*. Tarn's *Hellenistic Civilisation* has escaped the bibliography.

The *Loek* is the occasion of a curious conflict of opinions. The new fragment of Callimachus' poem surprised Mr. Barber by showing that Catullus's version is far from being literal, and Mr. Levens by showing that Catullus was a less free translator than was formerly believed. Presumably the verdict must go to the scholar who has himself translated Catullus into Greek.

Mr. Levens's trenchant pages on Catullus form the most memorable of the contributions on Latin literature, though one reads with interest in Mr. Huxley's section on Silver Latin poetry of a scholar who enjoyed Valerius Flaccus and of his hope that someone will reveal merit in Silius; certainly he has the sympathy of the shade of Addison, who enjoyed both. Levens has been fortunate in having a subject far more manageable in point of length than most of the writers in this volume. What he has produced could form a large part of an admirable introduction to an edition of Catullus. Though one may sympathise with a certain shortness of manner when he is discussing the exploitation of Catullus's love life, both by scholars and the less scholarly, it is difficult not to feel that if poem 79 is insufficient evidence that Lesbia was a sister of Clodius, then there are few statements about the ancient world which we are justified in believing.

It would be pusillanimous not to attempt to reach some conclusion. On the one hand, a surprising amount of fundamental work still remains to be done, not only editions of authors and of fragments which should make full use of knowledge available but even in some cases the elucidation of the MS. tradition. The output of new tools, such as lexica to individual authors and the Latin *Thesaurus* not mentioned in this work, is steady but slow, though Pauly-Wissowa approaches completion. On the other hand, in some fields of work there is considerable congestion. Though there are always outstanding scholars who enrich whatever they touch, a great many in reworking what has been worked before are reduced to the making of unlikely 'combinations' and to defending the indefensible. To give a single instance, surely to argue that Seneca's Tragedies were written in order to be performed is to waste ink. Striking advances arise from the discovery of new material or from developments in other branches of knowledge, from digging up Troy or observing medicine-men in Melanesia. Anthropology may not have contributed as much as once was hoped, but even recently the study of comparative 'enthusiasm' has thrown light on the *Bacchae*. No doubt such help will continue to be given, but it is difficult to think of fresh studies whose development might appreciably affect our understanding of antiquity—psychology, perhaps, if it became a real science, or Marxism, if it became compulsory. From the intensive study of existing

data, though truth will increase and error be scotched, though discoveries will be made which seem important to those intimately concerned with the subject in question, no spectacular development can be expected to emerge. As is often avowed in the pages of this book, the evidence necessary for a firm decision is not available. But new evidence does arrive in considerable abundance as more languages become readable; the discovery of material remains, both written and other, gives at least the hope of an inexhaustible future.

D. W. LUCAS.

**Scripta Hierosolymitana. Vol. I. Studies in Classics and Jewish Hellenism.** Edited on behalf of the Faculty of Humanities by R. KOEBNER. Pp. 144. Jerusalem: Hebrew University, 1954. Price not stated.

The present volume is the first of a series of studies projected by the Hebrew University which has had the happy idea of grouping together in each volume studies relating to a single subject. One naturally looks with some doubts on the appearance of a new learned periodical—there are already so many; but this deserves a welcome. The various aspects of the subject of the impact of Hellenism on Jewish thought are worthy of more attention than they have usually received. The aspects here treated are: a study by Y. Gutman of the fragments of a Jewish epic poet, Philo, who wrote a poem on Jerusalem, in which Gutman distinguishes Orphic influences. The few surviving lines, viewed as Greek poetry, do not cause one greatly to lament the loss of the rest. Tcherikower contributes a scholarly article on the meaning of the personal name Sambathion and its occurrences, apparently among non-Jews, but under Jewish influence. A. Shalit discusses the origin and meaning of *Κολή Συρία*, which he suggests is a corruption of a Persian administrative term, *Kūl-Aram*, 'All-Syria'. But would *kūl* be corrupted into *kolāh*?<sup>1</sup> Schwabe publishes an interesting elegiac epitaph from Gofna, which he convincingly shows to be authentic.

The remaining articles are of general classical interest. H. Rosen interprets the Lemnos stele with more courage than plausibility (and great ingenuity) as Greek. A. Fuks writes on the 'Old Oligarch', and R. Koebner, the editor, concludes with a masterly essay on the uses and development of the term *imperiūm*.

R. D. BARNETT.

**Voices from the Past. A classical anthology for the modern reader.** By J. and J. M. Todd. Pp. 350, with 9 plates. London: Phoenix, 1955. 30s.

In their editorial note to this anthology Mr. and Mrs. Todd explain that they have tried to present a panorama of the entire literature of classical times from 800 B.C. to A.D. 500, in which are included not only the well-known writers but a large number of the less famous; and they make it clear that the book is intended not only for those who have had no classical education but also for the student of the Classics who wishes to extend his enjoyment of them. The selection has clearly been made with love, skill, and enterprise. Alongside the main literary authors are found extracts from mathematical and scientific works, philosophy, the Septuagint, early fathers of the Church, and minor poets of the anthologies, each prefaced with a brief but useful summary of the author's life and work; and the whole collection sketches very successfully the variety of style, subject, and perception which the bewildering genius of the ancient world brought within the scope of literature in the spring, summer, and autumn of its progress. For the object the authors have in mind, it is hard to see how they could have done differently in reasonable compass, and it would be ungenerous to complain of omissions, for they have succeeded in illustrating very ably many aspects of the Greek and Roman *épêh*, to which Dr. Bowra calls attention in his Foreword. Mention should perhaps have been made of Euripides' experiments in romantic drama. Theophrastus should have had half a page for one of his characters, and Lucretius have been allowed to display a little of his atomic learning. But the editors were probably right in deciding to make no attempt to convey the underlying philosophy of, for example, the Republic.

At this point a doubt presents itself, that the very broadness of the selection which the authors have set out to make may weaken the book's appeal, both to classicists and to the general

<sup>1</sup> W. Brandenstein in *Anz. f. die Altertumswiss.* 1955 (Der Name Coelesyrien) has suggested not very plausibly that it is from *κόλας*, 'beautiful'!



reader. The classicist may find the selected passages of Homer, the tragedians, and the other major authors too short and sketchy for his taste; and the 'non-classic' may have trouble in finding his way without bewilderment amongst so many new and fugitive scents and sounds. If the one party suffers the pangs of Tantalus, the other will face the bewilderment of an outsize trolley of hors d'oeuvre, admittedly stocked with the finest ingredients. This does not mean the attempt was not worth making, and the enquiring reader will at least know where to turn for a fuller translation of an author who catches his fancy.

The translations chosen for the extracts come from all periods, and in their variety they afford the opportunity of contrasting and comparing the effectiveness of modern and older versions of favourite authors. In general, verse translations are printed for verse authors, but for Homer there are examples of Rieu, Lang Leaf and Myers, Shaw, Butcher and Lang, and Samuel Butler in prose, a company in which W. H. D. Rouse's free version might well have been included, for its vigour and sweep seem to win the hearts of new readers of Homer more readily than all the rest. For the tragedians, modern stress metres prove less effective in short extracts than do older styles; particularly in the one extract from the *Oresteia*, Agam. 1258-1411, where Mr. Todd's version seems much less satisfactory than either his own rhyming version from the *Prometheus* or Lewis Campbell's lines which precede it. Both editors are lively translators who experiment sensitively with a variety of styles, though among the sixty-nine passages they have rendered some suffer from their proximity to extracts from giants of the past. They are most successful in the shorter lyrics and elegiacs, which have the advantage of wholeness, in comparison with snippets of drama, history, or epic. Sometimes there is a false note—for example, 'O winsome spirit, loth to rest' for 'animula vagula, blandula'; or a mistaken emphasis such as

'take now these offerings of a mourner's giving  
In the sad ritual of an older day'

for 'prisco quae more parentum/tradita sunt tristi munere ad inferias' in Catullus 101, where *prisco* surely means 'time-honoured'. A pity that Macnaghten's versions of Catullus were not included, for he was past master at this kind of problem.

The extracts are preceded by a brief introduction, in which the writers rightly emphasise, amongst other influences, the growing importance of Christian thought from the end of the 1st century A.D. A fussily designed, though useful map of Greece and Italy and nine half-tone plates illustrate the text.

P. G. MASON.

**The Homeric Odyssey.** By DENYS PAGE. Pp. vi + 186. Oxford: The Clarendon Press, 1955. 21s.

To map the *Odyssey's* share in the Homeric Question would seem enough to fill six short chapters, even when, to the text of the Mary Flexner Lectures delivered at Bryn Mawr College, meaty notes and an Appendix have been added. This is all that Professor Page promises, and it is amply provided; the lucidity and rhetorical presentation make easy reading for the undergraduates for whom the book is avowedly intended, and there is probably no one who cannot learn something from the rich erudition and acute criticism. Little of value escapes his net (the omission of the 2nd edition, 1950, of M. P. Nilsson's *Minoan-Mycenaean Religion* is the most important); and if some of the theories discussed might have been left to a merciful oblivion, the picture of scholarship presented would be incomplete without some follies. As would be expected, however, the author is not content to survey past work. In spite of a Socratic pretence that he is following the argument whithersoever it leads, he knows very well where he is going, and by judicious selection and cumulative argument he builds up a body of established fact. The *Odyssey* was composed between 900 and 700 B.C., probably towards the middle of the period, a little later than the *Iliad* and in a different city of the Eastern Aegean, by a poet who did not know the *Iliad*. It came at or near the culmination of a very long tradition of oral poetry; the possibility that writing, though it played little or no part in the making of the poem, was used to record it while it was being made, is admitted, but the whole conception of a static poem in a standard text is entirely foreign to the memory-technique of verse-making. (The distinction between 'publication' and 'preservation' in writing might be made clearer.) The poem would not have been composed or preserved if there had been no audience for it, and either parts in isolation or the whole poem over a number of days must have been recited in court or market or both. During the Dark Ages the courts of rulers who, certainly in Lesbos and presumably elsewhere, claimed

descent from the heroes, are places where heroic poetry may well have been continuously composed and recited. But our *Odyssey* is derived from an Athenian manuscript of the sixth century, through its descendant or descendants which survived to be re-edited by Alexandrian scholars. Changes therefore may be expected, especially on its way to Athens and in the Athenian editing, and the four central chapters are devoted to finding them. The *Continuation* and the whole *Nekuia* were added, the Assembly in β was recast and the second Assembly of the gods introduced in the interest of partial recitation, and the main narrative was sporadically contaminated by versions in which the magical transformation of Odysseus was complete, the recognition by Penelope occurred before the fight, and Theoclymenus played a significant part; as well as by smaller episodes such as the *Moving of the arms* and the *Web*. But if the ghost of the poet saw his poem today, he would find that it 'remains essentially unspoilt—in the Eleventh Book improved — by the hands of time and chance'. Proof of so substantial an area of certainty would be a considerable advance; it is put forward as with authority and maintained with a panoply of learning, rhetoric, and ridicule; of all the arguments that the author could advance, these are the ones he thinks convincing; and it is on their cogency that the real value of the book depends. They are not all equally cogent.

There is room for only a few examples. Some of the arguments are surprising: (P. 12) *In some early version the drawing of lots decided who should be eaten next, because Odysseus would not have risked choosing helpers by lot, and then exclaimed, 'The very ones I should have desired to choose.' 'Then why not choose them?'* Yet in H a champion is chosen by lot, and although the Greeks tactfully gave Zeus a choice, the one they got was the very one they wanted; they could not have sent Aias into battle with the ill-omened words, 'Pity it wasn't Diomedes.' In the *Odyssey* the phrase objected to could never have stood in a version where it meant, 'Just the ones I wanted the Cyclops to eat.' It is hard to believe that such an argument is meant to be taken seriously. (P. 149) *If the poet had known the Iliad he would have used the common expressions in it appropriate to his subject.* But since he knew *καρπαλίμως*, he knew *καρπαλίμως* without using it, as well as the many other expressions which he used only once or very seldom; in fact, he was not a schoolboy composer, depending on tags from the *Iliad*. The author does not discuss the difficulties which his solution creates—the extent and character of the common element, the Athenian editor's respect for his poet's difference of idiom in the two poems, and the historical situation in which the East Greeks were in contact with the Phoenicians in the eighth century but isolated from each other until the late sixth. (P. 29) *When advice and execution are given as nearly as possible in identical language, it is axiomatic that the passage which comes first was composed first.* This is true only if we assume complete improvisation. Uneasiness caused by such reasoning is increased by the rhetorical overstatement. Many of the arguments would be less forceful if the quotations were in Greek. *μοχλὸς θάλασσης*... *χλωρός* is a suitable weapon, and would glow; to translate 'log' is to gully it. A man may be given the form *παλαῖος*... *γέροντος* and look *ἀλλοῖος* when his youth is restored, without being 'quite a different person'; and neither the swelling of withered muscles nor the growth of hair after a bath ('beautify') is natural. *οὐ πως ἐστὶ δέμας ἀκούσας ἀπώσσει* is not an abrupt refusal and does not contradict Athens's *ἐπὶ οὐδὲς ἐφορμᾶται γαμψέσθαι*, especially as in the interval Penelope has shown her unwillingness; the speech is difficult enough without this added complication. The card-index method itself exaggerates; the solitary Cyclops has neighbours in the same episode and the heroines, etc., create an immediate awkwardness, but most of the other difficulties would be heard days of reciting apart. All this throws doubt on the solutions offered, especially as the really strong cases (e.g. the *Continuation*) need no such support.

The Unitarian, if he is not satisfied with the surprising amount of genuine poem left him, will not lack targets and may gain new arguments from the lively proof that the Polyphemus story shows multiplicity not of authors but of stories. Peisistratids may object that sixth-century Athenians were not dots, and that a literate editor is the least likely person to write Mentès' odd speech or introduce Theoclymenus at length without giving him enough to do. Since we know little of other epics except that they were less good, we depend on the poems for our idea of epic technique, and it is at least as reasonable to argue that the transitions are late because they are neat and sophisticated as that the second Divine Assembly is late because it is a somewhat clumsy experiment. The problems remain problematic; sand blows over the 'green oases of comfort', quicksands submerge the 'solid ground', and there are traces of 'meretricious cosmetic' on the plain face of common sense.

D. H. F. GRAY.



**Der Odyssee-Dichter und die Ilias.** By A. HEUBECK. Pp. 102. Erlangen: Palm & Enke, 1954. DM. 7.50.

Dr. Heubeck first discusses the poetic characteristics which in the *Odyssey* are developed or adapted from the usage of the *Iliad*. This is of interest whether they should be attributed to poet or editor, but the author's object is to show that a poet created our *Odyssey* by imposing his personality on the pre-*Iliad* tradition which was his inheritance and the *Iliad* which was his model. He propounds the dilemma that the *Telemachy*, without the parts which clearly belong to the *Odyssey*, has too little substance for a separate epic, while if a later poet composed the *Telemachy* to turn an original *Nostos* into the *Odyssey*, we are back in the difficulty that the same poet was responsible for the neat narrative and the awkward joins. To the literary arguments, he adds a good discussion of Fate and the gods; in the *Iliad*, divine blinding and human blindness are inseparable, although characteristically Agamemnon puts all the blame on the gods and Hector takes all the blame to himself, and the gods in the end enforce *μοῖρα*; in the *Odyssey* men may offend and ruin themselves *ὑπὲρ μέτρον*, and the gods can only dissuade them (Aegisthus and the Suitors), or, with no disposition to sin, men may be brought by circumstances to offend (Odysseus and his crew). The poet of the *Odyssey* assimilated the *Iliad*; the poets of the *Cycle*, as far as is known, were mechanical imitators. The judgement of the Chorizontes is justified by modern arguments; and, whether or not the relation of the two poets was that of master and pupil, it is certain that the poet of the *Iliad* had many imitators but only one successor. The book is inspired by Professor Schadewaldt's treatment of the *Iliad*, and owes much to German scholarship, especially in the form of robbing the analysts of their arguments; works in other languages are almost ignored, although the author's approach is English rather than German. The case would be stronger if the Unitarianism were less indiscriminate. The resemblance, if there is one, between Π 168-97 and λ 235-327 is 'mechanical', and the comparison of the *ῥήματα* in Ω and the *σπονδοί* in ω shocks all poetic feeling, especially as the compulsive motives of the hero's excessive wrath and the enemy's nobility are lacking in the *Odyssey*. But in general the working hypothesis of a highly individual poet, on whom the *Iliad* was the chief formative influence, is maintained with effective moderation.

D. H. F. GRAY.

**Il poema di Ulisse.** By L. A. STELLA. Pp. xvi + 443. Florence: La Nuova Italia, 1955. L. 2300.

Professor Stella sets out to describe the historical, mythical, religious, political, and artistic background as well as to write an appreciation of the *Odyssey*. His book is divided into two parts, the first having three subsections dealing with the historical and cultural background, the 'literary tradition', and the religious setting of the poem. Any scholar today who writes a comprehensive work on even one of the Homeric poems risks being either tediously long or inadequately brief. Stella has reduced his material to manageable proportions by avoiding polemic. This, together with a vivid and enthusiastic style, makes for an admirably lucid and readable exposition; and if the text seems at times to put forward controversial views as if they were generally agreed, a corrective is provided by the four extensive bibliographies, totalling seventy-three pages, arranged in the form of a commentary on the arguments of the preface and first three sections of the book. This device makes it possible to mention and very briefly discuss the views of other scholars.

The influence of Autran is very marked in the first two sections, which deal with the historical background and literary antecedents. It is assumed that the centres of civilisation in the Aegean and Near East in the Bronze Age shared a common culture, a common outlook and political organisation, and a common literature. Stella has gathered a vast amount of material from Near Eastern sources, and seeks to show how faithfully the *Odyssey* reflects that civilisation and its ideals. Space compels him to be eclectic, and doubtless no selection can be completely satisfactory; but a standard of relevance that concentrates on the *Odyssey* to the virtual exclusion of the *Iliad* is bound sometimes to be misleading. One might have preferred some account of Mycenaean political geography or methods of warfare (the bow is a conspicuous omission) rather than a derivation of such fixed epithets as ἀρηϊεύς, βιογενής, θεοειδής from Mycenaean court protocol on the basis of similar forms of address to the Pharaohs. It would be difficult on this theory to account for the less fulsome, but equally fixed, epithets *βοῶν ἀγαθός*, *ξανθός* or even *παλόμενος*. Not less idiosyncratic is the confident identification of *Κήρυκος* with Hittites, or of the Trojan War in Hittite records because *Lesbos* (= *Lazpa*) was the rendezvous of Menelaus' fleet in III 169. Attarišiyāš

(= Atreides) is accordingly equated with Agamemnon. This section is concluded by an excursus on the date of the *Odyssey*. Stella argues, rightly, that the philologists cannot produce an absolute dating, but goes on to reject their labours as altogether irrelevant. He sees no reason from his understanding of the archaeological evidence to bring the date lower than the second millennium, and considers the *Odyssey* older than the *Iliad*.

Stella numbers himself among the *χαριζομένους*, but as regards the *Odyssey* itself he is a fanatical unitarian. Even the Nekyia finds acceptance. Sometimes, one feels, the 'analitici' are made to hold rather outmoded, even Lachmannian, views, and there is not much that would effectively counter the kind of criticism that Professor Page has recently made. Stella believes not only in a literate poet, but in a long literary tradition. (Linear B, was deciphered in time to be mentioned but not digested. The claim (p. 126) that here is an indication of the existence of a written literature has recently been rejected by Bowra.) This thesis is illustrated in the second section with a wealth of quotation from Near Eastern literature showing the impressive parallelism between it and the *Odyssey* in motif, artifice, and style. This is perhaps the most valuable portion of the book. Some may think Stella goes too far in holding that the poet of the *Odyssey*, surmounting the differences of language, stands directly in the stream of that tradition. He would deny, for instance, a fairly immediate origin in folklore for motifs in the *Odyssey* because they appear in, say, a Sumerian poem. The traditional stories of the Slavs and Germans are ignored. At one point indeed (p. 316) it is even suggested that their folklore is derivative from the Near Eastern literary tradition. The formulaic style naturally causes some difficulty. No less formidable, had it been mentioned, would be the tendency of words to have a fixed place in the verse according to their metrical shape. A paper by O'Neill in *Yale Classical Studies*, Vol. VIII, could have been usefully added to the bibliography on this topic. As the pioneer, Milman Parry naturally concentrated his attention on the formulae used for principal characters, but his arguments apply with equal force to any common notion, so that it cannot be said that Stella has disposed of the belief in oral composition by pointing out that Near Eastern heroes also have their fixed epithets. The argument would have force only if the Oriental tradition were indeed 'rigorously literate' (p. 105). Divergent texts are not unknown; and Albright, in an article mentioned by Stella (*AJA* 1950, p. 164), has stated his conviction that 'even in such literate regions as Egypt and Babylonia, or later Iran, India and China, literary composition was mostly oral'.

In the third and much shorter section on religion, Stella does not seek to describe Bronze Age beliefs, but to examine what use the poet makes of religious material. He finds the Odyssean Gods a device of plot. The motifs of the anger of Poseidon, the divine protectress, the council of the Gods, etc., are illustrated from Near Eastern poems from which Stella would derive them.

It is inevitable that in his appreciation of the poetry, which forms the second part of his book, Stella should approach it in the same spirit as he would the *Aeneid*. Fixed epithets are not due to metrical convenience but are allusive or otherwise, not superfluous. No mention here of ἀμόμονος Ἀλγίσσῃσιν, in connection with whom it was earlier (p. 17) said that such epithets were 'used with indifference'. Yet in the epithets of the sea there is a 'wonderful precision'. Three main themes are discerned in the poem which together illustrate the human sympathy of the poet that distinguishes him from the poet of the *Iliad* and the Orientals: the sadness of war, the lure of distant lands, the pathos of a return home alone and unknown. All are illustrated with copious quotation. In general, no fault is found in the *Odyssey*: at worst there are 'moments of lesser inspiration'. After the recent impeachments of Professor Page, it is refreshing to meet such ingenuous enthusiasm.

The numerous quotations from Greek and Near Eastern literature are all translated with elegance, apparently by Professor Stella. 'Nel mare fulgente' for *ἐν ἄλᾳ δῖαν* is surprising: 'scintillante' for *ὄρνυς* is more natural than our traditional 'wine dark', but overlooks the fact that the epithet is also applied to oxen. Misprints are virtually non-existent, but on p. 309 *ἡεροειδής* should have the smooth breathing. On pp. 105, 147, 148, Hattusaš should be read as the name of the city.

J. B. HAINSWORTH.

**The Ulysses Theme. A study in the adaptability of a traditional hero.** By W. B. STANFORD. Pp. x + 292. Oxford: Blackwell, 1954. 31s. 6d.

This little book is the fruit of wide reading joined to strong good sense and considerable ingenuity. The subject is the manner in which Odysseus, Homer's 'untypical hero', as the title of Chapter V calls him, has been handled by writers of



every age since the *Odyssey* was composed, and what modifications, favourable and unfavourable criticisms of the Ithacan's character, additions from their own conscious or unconscious nature, and so forth various writers have superimposed upon, or substituted for, those traits which are developed or at least indicated in the Homeric account. The author, of necessity, begins by analysing Homer, wasting no space over those who fancy that the *Iliad* and *Odyssey* are by different hands, still less over the dissectors of the two masterpieces. Rightly, he allows for a substratum of folk-tale, the figure of the Wily Lad (p. 10) or Trickster, whom, he suggests, Homer 'developed into the more sophisticated personality of his Odysseus'. The King of Ithaca is, of course, more than a trickster, indeed that side of his character was to do his reputation much harm, 'the nemesis of Autolycus' as it is neatly styled (p. 268 n. 9). How the developed figure is fitted into the heroic background, especially in the *Iliad*, is an interesting subject, and well handled in the first five chapters.

Then begins the long story of Odysseus' *Nachleben*, from the Epic Cycle on to the very latest works containing such a figure. Here rather too much space is devoted, especially in the last chapter, to certain *aegri somnia* of the last two or three decades. But on the whole the criticism is sound and good, the result of the author's own reflections, but free from any straining after originality at all costs. Documentation is provided in numerous notes, which, after the displeasing habit of too many publishers, are all put together at the end of the volume, instead of at the foot of the pages or at worst at the end of each chapter. Due space is allotted to such principal themes as the denigration of Odysseus on the Attic stage, the use of him by Stoics as an *utile exemplar*, his handling by Vergil and Dante, the anti-Homeric movement, which, in the works of Dares and Dictys, so influenced the Middle Ages, and the 'Ulysses' of the Revival of Letters, especially the part he plays in Shakespeare. Chapters XIV and XV deal respectively with 'the Wanderer' and 'the Re-integrated Hero', leading the reader into some strange corners of European literature. To examine them in detail would involve writing at least as many words as they contain.

Generally, S. is very sound and careful on points of fact, but a few little slips have crept in, as seems to be inevitable. On p. 71, cf. 255 n. 10, § 329 says nothing about arrow-poison; the Sutors are afraid of being served with poisoned wine, not shot at. P. 76, the brutality of the Cyclops did not go so far as to eat his victims alive, see 1289 ff.; he killed and, apparently, cooked them. P. 106, half-way down, surely 'rights' is someone's error for 'rites'. P. 150, I see no problem in the silence of Homer concerning Palamedes. Palamedes is a figure of folklore pure and simple, the supreme inventor, and as a rule Homer does not deal in folklore in the *Iliad*; for instance, he says nothing of the fire-brand in speaking of Meleagros. On p. 158 it is misleading to call so good a Platonist as Plutarch 'loyal to Stoicism'; the meaning is no doubt that he, like the Stoics, thought of Odysseus as an admirable figure. P. 282 (bibliography), it was not Andrew Lang but R. R. Marett who edited *Anthropology and the Classics*.

H. J. ROSE.

**The Life and Work of Sophocles.** By F. J. H. LETTERS. Pp. 310, with 1 plate. London & New York: Sheed & Ward, 1953. 18s.

This book, like *Ajax*, *Antigone*, and *Trachiniae*, is constructed on the 'diptych' principle, and may perhaps, like those plays, be felt to lack unity. The first four chapters, entitled *The Time*, *The Man*, *The Poet*, and *The Dramatist*, offer a background to the detailed discussion of the plays themselves, which occupies the second part of the book. It may be questioned whether the account of conditions in Athens during the poet's life-time, with its concentration upon domestic details, social and economic habits, statistics about population, and judicial and administrative machinery, contributes anything of value, in spite of its intrinsic interest for the general reader, to our understanding of Sophoclean tragedy. More useful would have been a sketch of Athenian intellectual life, with a glance at what men of culture were thinking during the last half of the fifth century. In the second chapter the author, after a judicious attempt to arrive at an estimate of Sophocles' character, has some sensible things to say about his religious beliefs, but it is difficult to agree with the conclusion that Sophocles is interested less in humanistic ethics than in moral theology. The most stimulating pages in the first part of the book are to be found in Chapter III, where, in a discussion of Sophocles' language, there is an acute and sympathetic analysis of the difficult Ode in *Trachiniae*, 830 ff. The author reveals here a highly developed feeling for the subtleties of poetic diction and a good ear for the overtones of Sophoclean imagery. His defence of

the puzzling *φάσμα* (misprinted *φείσμα* on p. 78) in 837 combines scholarship and imagination in a nice balance, and is, to the reviewer, convincing. He shows the same insight into language in analysing the alternation of the image with its original object in the Parodos of *Antigone* and in supporting the MSS. *κῶνις* in 602 of the same play by reference to the Guard's description of the dust storm in 415 ff. He is unquestionably right in stating that symbolism is deeply rooted in the poetry of Sophocles and that its due consideration may well occupy the scholarship of coming years. Professor Goheen, in *The Imagery of Sophocles' Antigone*, has pointed the way, and perhaps Mr. Letters may feel disposed to pursue the same method for some of the other plays.

It is not possible in a short review to do more than note a few things in the second part of the book. The author presents here summaries of the plays with critical discussions of the problems they raise. He sees Ajax as an egocentric representative of a Homeric shame-culture with no virtues but his heroism, who is saved, not in the sense that his honour is retrieved, but 'from the very real damnation of exile from the company of Hades'. In his remarks on the Prologue he fails to see in it one of the cruellest scenes in Greek Tragedy in its depiction of a goddess mocking a stricken human being. In dealing with *Antigone*, he appears to accept as genuine the demonstrably spurious end of Aeschylus' *Seven Against Thebes* and is perhaps wrong in regarding Creon at the beginning of the play as a full-blown tyrant whose edict has shocked the religious sense of Thebes as well as of the audience. He does well, however, to emphasise Antigone's truculence, and well, also, to reject the Aldine's attribution of 572 to Antigone, to see in 899 a reference to Eteocles, and to accept as genuine the whole of this celebrated speech, though we would have welcomed a less perfunctory paragraph on his reasons for doing so. He makes the good point that *Trachiniae* is saturated with primitive magic, but is it correct to say that 'Heracles absorbs us utterly from Prologue to Epilogue'? Far more absorbing is the character of Deianira.

Lack of space forbids further detailed remarks, and it remains to say that the general reader will find much of value in this well-written book, and the scholar may be stimulated by Chapter III to fresh thought on the language of Sophocles.

R. W. B. BURTON.

#### **The Plays of Sophocles. Commentaries, Part 1:**

**The Ajax.** By J. C. KAMERBECK. English trans. by H. SCHREUDER and A. PARKER. Pp. ix + 261. Leiden: E. J. Brill, 1953. Price not stated.

This is the first volume of the first full-length commentary on Sophocles to appear for some fifty years. Originally each volume was to contain a text as well as a commentary; but the appearance of A. Turyn's *Manuscript Tradition of the Tragedies of Sophocles* has caused K. to postpone the publication of a text, substituting for the present a list of the passages in which he differs from Pearson's reading.

A brief introduction deals with the earlier history of the legend, summarises the play, and replies sensibly to those who complain of its lack of unity. In the commentary, K. has made a laudable attempt to turn to account the discoveries and researches of the last fifty years. He is familiar with the literature of the subject produced during that period, though he seems to have no chance to go through English periodicals. He seems not to know Pearson's notes at *Proc. Cam. Phil. Soc.*, 1922, 14 f. (cf. also CR XXXIV, 1920, 57). The notes dealing with *Realien* are usually well-informed, and the treatment of the drama and its successive situations is sensible, if sometimes rather heavy-footed. There is little interesting discussion of points of grammar and syntax; and though Jebb's handling of metre is one of his principal shortcomings, K. has so far failed to make it good that there is hardly anything about metre in the book at all. In textual criticism K. is cautious to a fault. He steers clear, wisely enough for the most part, of most of Pearson's innovations, and in one or two places he does useful service by showing that a manuscript reading has been wrongly altered by most editors. But against this one must set numerous examples of 'conservative criticism' in the worst sense. K.'s natural caution far too often degenerates into an excessive timidity; he will mention two or more possible views and pass on with the remark that both are possible in places where a determined attempt might help one to decide in favour of one or the other. Both in textual criticism and in exegesis, K. is handicapped by the same deficiency. The truth is that, even after fifty years, no one can hope to produce a commentary on a play of Sophocles that can to any sensible degree improve on Jebb unless he possesses in a measure at least comparable with Jebb the exquisite feeling for style and language for which that great scholar is pre-eminent. This quality K., like most of us, falls far short of, as some of the quotations and remarks below will help to show. He has produced a book which is certainly worth consulting by



anyone who takes a special interest in the play, but which comes nowhere near replacing Jebb's as the most useful commentary.

I think it is a pity K. printed his book in English. Its language can, indeed, easily be understood, but the laboured and undistinguished style is obviously that of a translation, and contrasts most unfavourably with Jebb's prose. In this country the book costs more than four pounds.

40 δυσλόγιτον... χερα is rendered 'ill-considered hand', because 'verbal adjectives in -τος composed with δυσ- are seldom active, especially in classical Greek'. The notion expressed by such a compound may be too vague for either 'active' or 'passive' to apply to it; cf. δυσίριστος, δυσθήρητος in this author. 51-2 The genitive χαρῆς must depend on δυσφάρος... γνώμης, which will not otherwise convey an intelligible meaning. χαρὰ is a stronger word than ἡδονή, and is here 'exultation'; cf. A. Th. 442 with Wilamowitz' note, E. Alc. 1125. 55 φόνον cannot mean 'blood' here. 59 ποτῶν must here refer to mental wandering. 79-82 K. draws attention to a very apt comment in the scholia. 107 Does πλέον really mean 'advantage' here? 163 Is it certain that προδιδάσκων cannot imply gradual teaching? Tr. 681 is certainly a counter-instance; but see A. Nub. 476 γνώμης is not 'views' but 'proper sentiments'. 169 Even if E. Her. 83 is held to protect the breach of synapheia, the δ' which Dawes inserted is needed to avoid an undesirable asyndeton. 189 This genitive requires discussion. 190 Pearson has shown how the corruption postulated by Morstadt's emendation may have taken place (loc. cit., 20). 210 The objection to 'following the traditional reading' is that the usage involved has no parallel in tragedy. 225 'The reading φασάν (without a comma) and ὑποκλιζόμενον is quite possible (as is indeed found in most MSS., although this is not conclusive)'. I suppose the concessive clause shows that Wilamowitz and Housman have not lived in vain. 231 The citation of a fragment from Euripides' Oeneus is not merely irrelevant, but ridiculous, since as K. himself is aware half of it is von Arnim's supplement. 242 μόνον διπλῆ. Why does διπλῆ 'heighten the pathos'? 261 True, the use of παραπράσσω supposed by the scholiast is unparalleled; but that of παραδράω at o 324, cited by Campbell, is relevant. 297 K. keeps εὐκριν. 305 'The reading of L, ἀπῆξας, is not satisfactory.' So several people have thought; but why not? 324 'Tecmessa's anxiety is expressed by the dactyle (sic) in the third foot.' 329 K. disagrees with Pickard-Cambridge about the use of the ὁσώλημα, but does not say why. 330 K. keeps φίλοι, placing a comma before it and taking it to be 'a pathetic appeal'. P. 84 καρδίας at A. Th. 288 is no evidence for ἄλγος at Aj. 359; see Maas, *Griechische Metrik*, 120. 360 K. rightly excludes the unnecessary conjecture πικρῶν. ποιμαίνειν is so well attested in the sense of 'tend, look after' that Jebb is clearly wrong in insisting that ποιμένων would have to mean 'chieftains'. 379 K. is right in refusing to change πάνθ' ὁρῶν, and perhaps also in taking ὄργανον as an adjective. 386 (on οὐχ ὁρῶς ἴν' εἰ κοῦσθ) 'The frequent occurrence of this kind of expression in Sophocles (absent in Aeschylus, si quid video) is due to the importance of the tragical situation of the person.' 398-400 K. paraphrases (following Jebb) οὐτε γὰρ (εἰς) θεῶν γένος οὐτε εἰς ὀνείων τινα ὁμιλῶν ἀνθρώπων βλέπειν ἐπὶ ἀξίος (εἶμι). It would be simpler to paraphrase οὐτε γὰρ θεῶν γένος οὐτε ὁμιλῶν ἀνθρώπων (γένος) βλέπειν εἰς ὀνείων τινα ἀξίος (εἶμι) (for the construction εἰς ὀνείων τινα cf. Ph. 111 with Jebb's note; the sense will be that of the common locus τί δὴ? μοι γῆρ κέρδος; see Page on E. Med. 145). 405-6 K. reads τοῖσι δ' ὁμοῦ πῆλας and translates 'and I am in the company of these', i.e., the slaughtered beasts, ὁμοῦ is omitted from the translation, presumably because of its awkward similarity in sense to πῆλας; nor does K. justify the unusual ellipse of εἶμι which this involves. 447 K.'s emendation of ἀπῆξαν to ἀπῆξαν would be unnecessary even if this form of the aorist of ἄγω were found anywhere in tragedy. 511-12 There is no need to take διοίσει as passive. For the sense of ὁπὸ see LSJ s.v. A II 2; not 'under the power of', but 'by the agency of' is the sense here. 516 K. keeps ἀλλ' ἡ μοῖρα, thus producing an absurdly abrupt effect. 540 Th. 1030 is quoted as though it were by Aeschylus. 600-4 K.'s method of dealing with this locus desperandus is to set out Wilamowitz' emended version, and then to introduce another with the words, 'There is another reading possible'. The range of possibility here is a good deal wider than this implies. 636 K. reads ἀπιστα. Like Pearson (loc. cit., 24), he sees that we should be better off without δέ. I suggest changing this δέ to εἰς; for the idiom involved, see Jebb on O.C. 563 and O.T. 1380. Probably we should read ἀπιστος, which is in the L. scholia; but εἰς πατρίδας γενεῆς ἦσαν ἀπιστα seems to me possible. 699 K. rightly reminds us that the Μῦσα of P. Oxy. 1615 is not certainly the right reading. The Suda article s.v. Νῦσα, which as Pearson saw (loc. cit., 24-5) derives from the scholia on this passage, obviously consists of two notes, one presupposing the reading Μῦσα and the other Νῦσα. 780 Would the reading τοσαῦτα really imply

that Calchas' speech had been a short one? 784 For the use of γένος here K. ingeniously compares that at Z 180, l 538. 832 'Syntactically it is most natural to take πομπῶν predicatively'. I think not. 869 No attempt is made to tackle the difficulty; the rendering 'And no place knows that I have learned along with it' is quoted as though it made sense. K. ignores Rattenbury's remarks in *Proc. Cam. Phil. Soc.*, 1935, 6-7. 889 'It seems more natural to consider οὐρα δρόμος as dependent on πῆλας and to take the intransitive πῆλας in the same way as the transitive in such instances as κράτει δὲ πῆλας (Pind., Ol. I, 78)'. I do not think it seems natural. 892 K.'s suggestion that παρούλος here, as at *trag. adesp.* 93 N<sup>2</sup> = *com. adesp.* 1254 K., means 'ill-sounding' is attractive. 907 'περιπτερές: the adjective is passive: ὡς περιπτερούμενος'. See CR II (N.S.), 1952, 133. 921 K. keeps the reading of the MSS. and translates 'Oh may his arrival be timely, if come he does.' Ignorance of Pearson's remarks (*loc. cit.*, 27) is no excuse. 924 In spite of Jebb's warning, K. supplies ὦν, thus assuming an impossible ellipsis and reducing the line to an appalling flatness. 946-8 K. takes τῶδ' ἀχ with ἀνολγῆτων, which involves a very curious word order. 1011 K. wisely accepts L's ἰαῶν. For the sense, see LSJ s.v. ἰαῶς II: 'sts. almost = ἰαρός'. 1026 Jebb rightly takes the aorist as 'instantaneous' and translates, 'Now seest thou...?' But Ajax, K. objects, is already dead, so εἶδης must refer to the past! Can he have noticed that Jebb in his note points out that we have here 'a rhetorical apostrophe to the corpse'? 1051 Jebb renders ἦντιν' αἰτίαν προδίδεις by 'what reason thou pretendest'; and K., who seems to think that 'pretend' can only mean 'allege falsely', takes pains to show that προδίδειν never means this. 1054 K.'s parallels do not explain the difficult ζητούντες; better Pearson, *loc. cit.*, 27. 1085-6 There is a difficulty in λυπώμεθα; see Pearson, *loc. cit.*, 28. 1090 τῶδ' is rightly taken to mean 'grave'. 1203-4 'Of course the chorus do not complain that they do not sleep well. They are deprived of the τέρψις ἐρώτων'. Accordingly K. prefers, 'with most MSS.', to take the first ἐρώτων with τέρψις and to place δ' after the second. No doubt he thinks the resulting text good poetry; but it is surprising that he does not realise that on active service lack of sleep may be an even greater bore than lack of company. 1295 Why must ἑπακτὸν ἄδρα mean 'foreigner' and not 'adulterer'? The basic meaning of the word is 'brought in from outside', which would suit either interpretation, and the second one goes better with this context.

HUGH LLOYD-JONES.

**Sophocles and Pericles.** By V. EHRENBURG. Pp. xi + 187, with 3 plates. Oxford: Blackwell, 1954. 25s.

The relations between Sophocles and Pericles, says Dr. Ehrenberg, have hardly ever been 'comprehensively discussed'. Seeing that apart from a few anecdotes we have no direct information about them, this is not really surprising. But E. thinks that the *Antigone* and the *Oedipus Tyrannus* contain much oblique criticism of Pericles.

E. anticipates that this thesis will encounter some opposition, and tries to disarm this by means of an opening chapter in which he discusses the principle on which his enquiry is to proceed. Historians, he says, have a right to use tragedy as part of their material; he mentions a few undoubted instances of allusions in tragedy to contemporary events; and he goes on to argue that tragedy reflects certain trends in contemporary thought. The first two propositions no one will dispute. Historians certainly have a right to use tragedy, provided the facts they claim to find in it are really there. Nor will anyone deny the presence of some unmistakable contemporary allusions, such as that to the Argive alliance in the *Orestia*, which E. instances. The third proposition is more dubious. In one sense tragedy obviously reflects contemporary thought, since it was written in the fifth century and not in the heroic age in which its action is located. But is the fifth-century thought which it reflects necessarily that of the most advanced kind? Is it safe to credit the tragedians with the familiarity with and interest in contemporary speculation that is commonly assigned to them? Or does tragedy, close as it is to the spirit of the epic and its mythology, reflect rather the thought of ordinary, and therefore conservative, contemporaries? And if tragedy does concern itself with the working out of moral problems, does it ever try to work out moral problems arising from contemporary politics?

The attempt to answer this question would require a long investigation, for which there is no space here. But it will be agreed that there is one type of reflection of contemporary thought which scholars have often tried to find in tragedy, but without basing their opinion on any secure evidence. This is the *cryptic* introduction of criticism of contemporary persons or beliefs which the initiated among the audience, it is thought,



will have been quick to recognise. It would be unwise to lay it down *a priori* that no such thing can have occurred. It lies in the nature of the case that such a criticism would be hard to recognise; and though one may feel strongly, as I do, that its presence in a play meant for public presentation would be surprising, to lay down this principle before examining an alleged instance would be to beg the question. One can, however, and indeed one must require that no alleged instance of this phenomenon can be believed in unless concrete and objective evidence can be cited in its favour. And if no such evidence can be cited, one must conclude that the instance in question rests on conjecture, not on proof.

In the second chapter E. convincingly explains that the 'unwritten laws' of *Ant.* 454 f. are the laws of the gods. Pericles, in the Funeral Speech (Thuc. ii 37, 3), uses the same expression of the human conventions which public opinion enforces. E. thinks that this reflects a fundamental difference between the religious poet and the sceptical and humanistic statesman. Pericles is said by the writer of [Lysias] vi, 10 to have used the term in referring to the laws which the Eumolpidae followed in their interpretations; but E. reasonably enough explains the difference between these two Periclean uses as due to the difference of the contexts in which they occur. Pericles, he concedes, may well have thought the rituals of the mysteries had a useful part to play in the life of the community; he was the friend not only of Anaxagoras the philosopher but of Lampon the prophet. There is, therefore, he concludes, no real inconsistency. It is curious that he insists that the different uses of this expression in the *Antigone* and the Funeral Speech must reveal a radical difference of outlook between the users. How does he know that Sophocles, speaking in a context like that of Thuc. ii 37, 3, would not have used it just as Pericles does, or that Pericles would have refused to acknowledge or obey the 'unwritten laws' of the *Antigone*? The pseudo-Lysian passage, for what it is worth, rather suggests that he would have done so; and Pericles was the friend not only of Anaxagoras but of Lampon. But E. has chosen to assume that the friendship with the philosopher, and not that with the prophet, is symptomatic of Pericles' real character.

In Chapter Three E. argues that Creon in the *Antigone* is a representative of 'State-absolutism', 'living in a world which has no room either for human magnanimity and greatness or for the unwritten laws of the gods, because the state has become an instrument of totalitarian politics and man-made standards, because moral issues have become the result of the rational and intellectual autonomy of man'. This language, borrowed as it is from the vocabulary of contemporary publicists, goes beyond the facts. Do the epic legends contain no other instance of regal arbitrariness? E. deals at some length with the ode beginning at 332, in which he thinks the Chorus is 'combating any daring deed, the unknown's breach of Creon's decree as well as the decree itself' (my italics). One has only to read this ode in its context to see that the second assumption is gratuitous. Nowhere in his treatment of the *Antigone* does E. face the difficulty of showing how the tyrannical conduct of Creon resembles the behaviour of Pericles, whose proudest boast was that none of the citizens had put on mourning through his act. One has to conclude that the criticism is levelled against absolutism in principle. The same must be true of that in the *Oedipus*: Oedipus, E. writes, 'represents a far wiser and more moderate type of ruler, but in the more important aspects the position is the same'.

In the fourth chapter E. sets out a useful review of the known facts of Pericles' career; all his stratagems are enumerated. In the fifth E. discusses the various expressions used to describe Pericles' unique position. Here, and in an appendix, he argues at some length that the reference to Creon as *στρατηγός* at *Ant.* 8 was meant as an unmistakable reference to Pericles through the title of the office which he held for such long periods. Now on his first entry Creon makes a deliberate, formal announcement that he has now assumed the regal power (162 f., esp. 173-4). This is the first that we, or anyone else, know of his accession; so what could be more natural than for *Antigone* in the opening dialogue, knowing only that Eteocles is dead and that Creon has taken over the command of an army still mobilised, should refer to Creon as 'the General'? Why should E. devote so much space and emphasis to such a trivial and flimsy argument? Because it is the nearest approach to a concrete and objective piece of evidence for his theory that he can muster. And without such concrete and objective evidence, theories like his, difficult as they must be to refute conclusively, cannot acquire any degree of positive probability.

The sixth chapter is a detailed discussion of the tribute list for 443-2, the year in which Sophocles was probably chairman of the Hellenotamiai. E. concludes that in this reassessment year the board did important work. Why, then, if Sophocles was an opponent of Pericles, did Pericles allow him to be chosen?

E. explains that he was 'not a party man', and that it was only when politics changed their character under the impact of new, and, as he feared, dangerous ideas that 'his deepest feelings could be stirred'. E. thinks they may have been stirred by his experiences as Hellenotamias, as he holds that the likeliest date for the *Antigone* is the spring of 442. Its criticism of Pericles was followed by another in the *O.T.*, which he dates 'soon after Pericles' death'. The plague at Thebes reflects the plague at Athens. In the seventh chapter E. 'tries to gather the various threads of our investigation and to strengthen its conclusions'. He explains that it is 'an attempt to see less the perfect outlines which strike the more remote onlooker than the tension and the working of the minds'. A brief final chapter offers a further summing-up. Appendix A deals sensibly with G. Thomson's attempt to connect 'unwritten laws' with the mysteries; Appendix B returns to the question of whether Creon was a general; and Appendix C discusses and rejects the contention of a Mr. N. O. Brown that the *Ajax* 'reflects the emergence of a new bourgeois system of values'.

Even if the *Antigone* and *O.T.* contained more specific evidence in favour of E.'s view than the passage in which Creon is called a general, I should not find his case convincing. He assumes throughout that in terms of 'the rivalry between the old-fashioned religious and the modern scientific mind', Pericles 'in his sincerest thoughts and feelings was on the side of reason and science'. Certainly some of his friends were prosecuted for impiety; but we do not know that Pericles himself shared their alleged opinions. Nothing in his known conduct seems clearly analogous to Creon's defiance of the unwritten laws by refusing burial to a corpse, or to the disbelief in oracles for which E. thinks that Iocaste, and in consequence Oedipus, is punished. The possible application of these plays to Pericles must therefore be restricted to such warnings against tyranny and scepticism in general as they contain. And since we cannot be sure that Sophocles thought Pericles to be either a tyrant or a sceptic, we cannot regard E.'s theory as more than a vague speculation.

The book is written in English that would do credit to a native; the presentation is admirably clear; and the thesis is illustrated with all E.'s usual learning, which is displayed to particular advantage in the treatment of details incidental to the main theme. Unfortunately this only serves to throw into relief the lack of evidence for the book's conclusions.

HUGH LLOYD-JONES.

#### The Political Plays of Euripides. By G. ZUNTZ.

Pp. xi + 157. Manchester: University Press, 1955. 18s.

Euripides' *Heraclidae* and *Suppliant Women* have for a long time been regarded as weak plays and patriotic show-pieces. More recently, this view has been, at least to some extent, contested. (We may be allowed to set apart Norwood's equally ingenious and absurd dissection of the *Suppliant Women* into two.) Dr. Zuntz, in the first and main section of his interesting little book, makes the two plays the objects not only of a penetrating interpretation, but also of almost unqualified aesthetic praise. Against some of the judgements of earlier scholars he is obviously right, but sometimes his enthusiasm leads him too far. That is particularly so with the *Suppliant Women*. He tells us that the second part of the play 'lifts the *drâme à thèse* into the sphere of tragedy'. It may be doubted how far the first part deserves that title, since the priggish Theseus is a rather peculiar protagonist of what I assume to be the *thèse*: praise of democracy and of Athenian loyalty to the common laws of Hellas. And the second part? Is the story of Capaneus and Eudæne with its mixture of satire and melodrama truly tragic? Are the boys carrying their fathers' ashes more than a moving spectacle? And what about Athena's awkward warning to Theseus not to be too easy-going with Argos or her justification—in this play of war-weariness—of the future war of revenge by the Epigoni? But perhaps we read more into the author's words than he intended to say. For he sums up by calling the two plays 'a playwright's paragon' for which it is enough to have expressed the idea 'of a rationally ordered fellowship of all men'. With this somewhat vague definition, the 'timeless essence' of the two political plays is characterised. Drawing in his interpretation with genuine passion on the experiences of our own time, Dr. Zuntz succeeds in digging deep into the general human foundations of the two plays, and thus is able to raise their meaning to an unexpected height, higher perhaps than most of his readers will be prepared to accept. Even he, however, recognises their historical character as their predominant feature, and it is in the chapter on 'Their Place in History' that the reviewer feels most in agreement with the author. Dr. Zuntz radically, perhaps even a little too radically, refuses to accept 'allusions to contemporary events', while he at the same time allows for a full share of tragedy in the general *Zeitgeist*, even in the particular spirit of a particular date. Thus the dates of the



performances of the two plays become important, and he makes a good case for spring 430 as the date of the *Heracleidae*, and (rather less convincingly) spring 424 as that of the *Suppliant Women*. These dates roughly coincide with the results of metrical statistics, though the author prudently takes these as a secondary confirmation only.

The second part of the book begins with a discussion of the geographical scene of the *Heracleidae*, which is explained by the transfer of an originally Marathonian story to Athens itself. The rest of the book consists of textual criticism which seems on the whole lucid and sound, and of an interesting chapter on 'The Tragic Hypothesis', dealing with a somewhat neglected part of our tradition of Attic tragedy. The whole book is stimulating and an important contribution to a discussion which is not likely soon to reach agreement.

VICTOR EHRENBURG.

**A Concordance to Euripides.** By J. T. ALLEN and G. ITALIE. Pp. xi + 686. Berkeley & Los Angeles: California University Press (London: Cambridge University Press), 1954. £3 15s.

It is recorded that, on the title-page of his copy of Beck's *Index accuratus et copiosus verborum formularumque omnium in Euripidis tragædiis integris et deperditarum fragmentis, nec non epistolis, occurritum*, Housman wrote 'Liar and slave'. The rebuke was well merited; Beck's index is infuriatingly inadequate, and the Euripidean scholar will turn with sincere gratitude to its monumental successor.

The concordance has taken nearly fifty years to complete. Professor Allen, unhappily, did not survive to see his life-work printed and published; hence the major portion of the book went to press without the benefit of his final revision. That Dr. Italie is responsible for only four letters, AMNO, is due solely to the mischances of war, which separated him for over five years from all collaboration in the project.

The scope of the work is comprehensive: it aims to include full references for all the extant plays and fragments, whether handed down to us on parchment or papyrus; to give the generally accepted text, and to mention significant manuscript variants, with some of the more plausible conjectures. So ambitious a project deserves detailed criticism, and the comments which follow are meant in no captious spirit.

(A) METHOD. One wonders why the system of a *Lexicon* was not preferred to that of a *Concordance*, if so much labour was to be expended upon a Euripidean reference-book. The reply to this would have occasioned even more labour is not entirely convincing. The book, in its present form, represents an uneasy compromise between the two systems: the editors have in fact grouped together the passages related by similar meaning or construction, but have provided the information *guttatim* under each different inflectional heading. This arrangement is most inconvenient, particularly in the longer entries, where the disposition of so much material under so many irrelevant headings obscures the basis of classification. The scholar, for example, who is rash enough to consult the fifteen pages devoted to δ, in an effort to discover how often, and in what circumstances, Euripides employs the article as a demonstrative pronoun, will find the instances neatly grouped in about twenty different places. He will be more fortunate, however, if he merely wishes to know with what words Euripides uses δ in crasis—far more fortunate, incidentally, than if he wishes to gain the same information concerning καί.

The editors have rightly decided that 'to cite every instance in an author's work of δέ, καί, and the like, and even of some others, is wasteful pedantry.' Nevertheless, we should have been grateful, in the entry καί, for instance, for more numerous subdivisions based upon syntactical and interpretational considerations.

By and large, in the Concordance as it stands, the editors have taken a great deal of trouble in classifying varied usages, and even, in some entries (cf. αἱ, ἐν, and πρὶν), in explaining how they have classified them; the more general system, however, of marking subdivisions merely by a light dash, tends to obscure the extent and purport of their categorisation. A more liberal use of full-face type, and of Latin explanations, would have added immeasurably to the work's utility. The system, again, of recording the instances, within each subdivision, in alphabetical order of plays, is inconvenient for the scholar who wishes to check his references; it would have been a better plan to follow the order of the plays in the Oxford Text.

(B) CONTENTS. Two rather surprising gaps are apparent:

(a) Papyrus readings, in the extant plays, which have been published since the appearance of the major editions of those plays. Thus, in the *Orestes*, which the reviewer has used for

checking purposes, the Concordance fails to mention συμφοράς (for συμφορὰν at 61), <κ>αὖτων [supplet] (for φρονῶν at 216), αὖτις (for αὖτις at 231 and 910), σύλλεκτρος (for δούλεκτρος at 508), βρόχους (for βρόχους at 1315), δόμοις (for δόμος/δόμοις at 1335). Nor are we told that papyrus support the Σ's νόσφ at 224, and Μ's πετρούμενος at 946. By some chance, however, δγ' (for δαλ' at 1340) is included. [Papyri nos. 293, 298, 299, 303, in Pack's list.]

(b) Readings of H, as published in Spranger's collation (CQ 1938). Thus there is no mention of H's important reading γ(ε) εἶπας at 188, of its confirmation of Valckenae's ἀστούς at 906, or of its support for Μ's δισσώ at Hec. 897.

(C) CONSISTENCY. The recording of variants often appears to be somewhat aimless:

(a) The widespread lack of uniformity in recording manuscript readings may be illustrated by one, in itself, unimportant example. When, at *Orest.* 181, the manuscripts read διοχόμεσθ' οἰχόμεσθ' for διοχόμεσθ' οἰχόμεσθ', we are informed of this fact s.v. οἰχόμεσθ', but not s.v. διοχόμεσθ'. In the case of more important manuscript variants, which the editors have usually been careful to mention not only in the entry of the word they favour, but also in that of the word they consider less probable, the choice of the relevant entries is not always judicious: thus, when, at Hec. 897, δισσώ (now supported by H) μέριμνα (? μέριμνα) is one variant on δισσώ μέριμνα, this fact should be reported s.v. δισσώ, and not merely s.v. μέριμνα. So, also, in the case of the conjectures they quote. If, for example, Scaliger's εὐτραφέστατον (adopted by Murray), at *Cycl.* 380, was deemed worthy of mention s.v. εὐτραφής (and s.v. πάχος), it surely merited mention s.v. εὐτραφής. I note that a conjecture of Allen's is mentioned only under the word conjectured (s.v. ἀσθενέω).

(b) There are some surprising omissions and inclusions. A mere *lapis calami* in one manuscript (A) at *Orest.* 210—ἐπικούρου νόσου for ἐπικούρου νόσου—is recorded, but we are given no warning at all that γόνον and δόμον are manuscript variants in two places (177, 1038) in the play (in contexts where the choice between them is difficult), and that δόμον for γόνον has been plausibly conjectured by Kirchhoff in a third place (82).

(c) The attitude to habitual manuscript confusions is negligent. One would assume, from looking at the entry on πλεῖρον, that the editors meant to record all the instances where πλεῖρα (fem.) is a v.l.; in fact, however, *Orest.* 223 and 800 (and some others) are listed there without any indication of these manuscript variants—though 223 is also mentioned s.v. πλεῖρα (fem.). Again, when so many examples of πρ-εἰ and συ-εἰ variants are given, it is misleading to omit some of the cases. Nobody, it is true, would wish to see the concordance turned into a vast repository of manuscript *quisquiliæ*: it is consistency in the treatment of variants, rather than completeness, that the reader requires. It would have sufficed, in many cases, to give one general indication of the manuscript practice—cf. the heading οἰκτῖρον (εἰρω vulgo codd.); also, incidentally, σῖγα (σῖγα vulgo codd.) and Ἀγαμεμνόνιον (Ἀγαμεμνόνιον vulgo codd.) might have been mentioned.

(d) In the marking, by square brackets, of 'passages regularly condemned as spurious', there is no sort of consistency. *Orest.* 933 is marked as spurious s.v. πάλαι, Πελασγοί, but not s.v. Δαναοί, δεύτερον. [No mention of Δαναοί δὲ δεύτερον (recc.), which is surely right, whether the line is interpolated or not.] *Med.* 87 is not marked as spurious under any word contained in it except δικάως. *Alc.* 208 is marked as suspect or spurious under every word contained in it except κῆλον.

A great deal of this inconsistency is due, it seems, to an error in the method of preparation. If the editors had edited their text before the slipping process, to the extent of deciding what lines were 'generally regarded as spurious' and what variants and conjectures were worth mentioning under each entry, and had carried this information on to the slips, they would not then have needed, after the 'alphabetizing' of the slips, to make separate decisions as each word of a line cropped up. This method would have saved them a lot of work, and would have obviated a whole host of anomalies.

(D) MINOR ERRORS. Inaccuracies and omissions in the quotation of variants are, as has been stated, quite numerous. But the citing of line numbers is remarkably accurate, and the listing of instances remarkably complete. This is the Concordance's major merit. Misprints are rare, and, in most cases, unlikely to mislead. A few necessary corrections, however, have come to my notice; reference is to page, column, and line: 91.b.24: add 'IT 658'. 189.b.18: add 'HF 1416, 10 280'. 189.b.35: delete '10 280'. 251.b.3: add 'IA 1207'. 286.b.36: delete 'Or 531'. 286.b.42: add 'Or 531'. 313.b.31: add 'Hi 378' and delete 'Hi 1448'. 349.a.42-3: delete 'Hi 476' etc. 349.b.20: add 'Hi 476' (κῆλον). 386.a.39: alter the heading 'Μενέτω' to 'Μενέτωρ'. 543.a.25: alter 'Su 481' to 'Su 841'. 656.a.16: alter



'Hi 979' to 'Hr 979'. 671.b.1: alter '1342' to 'IT 1342'. 671.b.5: add 'Su 539'. The lines 306.b.24-31 should be deleted: they are a dittograph of the preceding eight lines. Should the reader wish to find the many places where *συμπός* occurs without the variant *μυρός*, the Concordance will not help him: from *συμπός* he is referred to *μυρός*, and at *μυρός* are recorded only those instances of *συμπός* where *μυρός* and *μυρός* are variants. On page xi of the Introduction, 'SMA Stobaeus cum codd. MA consentiens' should be altered to indicate that S, M, and A are the sigla of the three Stobaeus manuscripts.

The statements on the dust-jacket are pretentious and ill-informed.

The above comments should not be misconstrued. They are meant, in the main, as a warning to those who will use the Concordance, not as a rebuke to those who have compiled it. The criticisms of their method of arrangement can scarcely be gainsaid: doubtless they realised its shortcomings, since Italic's own *Index Aeschyleus* is built upon a different plan. But the other criticisms are criticisms of detail, and, as such, are not of great importance. *Λεξικά* which are *εὐχαῖς* *δμοῖα* never get past the *ἀ-ὠ* stage: the Concordance has been finished. The sheer labour of amassing and sorting the material must have been immense, and, subject to the qualifications mentioned above, the task has been very well done. The Concordance is remarkably accurate and full in the quotation of instances under each heading, and the editors have done far more than merely list their examples. They have arranged them with unobtrusive, but thorough, efficiency. Every student of the Classics should be grateful to the University of California for financing this work, to the Oxford University Press for printing it with characteristic accuracy, to the editors and their assistants for their industry and scholarship. No Classical Library, and no advanced student of Greek drama, should fail to purchase it.

G. A. LONGMAN.

**Agathon.** By P. LÉVÊQUE. Pp. 176. Paris: 'Les Belles Lettres', 1955. Price not stated.

This is a readable, leisurely study of the tragic poet Agathon, and puts together the little that we know about him from the *Thesmophoriazousae*, *Symposium*, and other sources. The author discusses Agathon's family, his private life in the rich Athenian society which cultivated the sophists, his attainments in the theatre, his departure for Macedonia, and then the details of his work: the influence of the sophists and the rhetoricians is illustrated both from the speech in the *Symposium* and from the fragments of the tragedies; finally, he is shown to have been a musician of the new school. Although he has nothing very new to say, Lévéque deals sensibly with the problems, and his work is well documented. Sometimes he goes too far; Agathon was effeminate, but the fact that he wears female clothing to write a woman's part (*Thesm.* 136 f., 218 f.) is no evidence of this; nor can Agathon's praise of Antiphon's Apology (*Ar. EE.*, 1232 b 7) be used as evidence of Agathon's courage, since we do not know that it was public. On the problem of the *Anthos* or *Anthus* Lévéque decides for *Anthos* in the sense of flower and believes that this was his first play, alluded to by the flower words in the *Symposium* speech; he argues that an *Anthus* based on a folk-tale is excluded by a strict interpretation of Aristotle; but perhaps Aristotle's 'invented' is not much more than the opposite of his 'well-known' (= in the heroic tradition). The other major problem is the date of Agathon's introduction of *embolima*; Lévéque stresses the importance of this for the structure of tragedy and believes (with Flickinger) that Aristotle deduced it from Agathon's text, which showed *ΧΟΡΟΙ* instead of a chorus (he might have noted the papyrus evidence for this practice in tragedy, cf. most recently *Hibeh* 2, no. 174). The information may rather have come from the records, as Aristotle's words seem to imply a general change of practice starting with Agathon. One choral fragment survives from Euripides' *Archelaos*, which was produced in Macedonia, and his last plays produced in Athens, like the *Oedipus Coloneus*, have normal choruses. According to Lévéque's chronology, Agathon died in Macedonia in 401; if the *embolima* were produced in his last years we must assume that his Macedonian practice affected subsequent Athenian practice; if they were produced in Athens they were contemporary with the *Orestes* and *Philoctetes*, which seems unlikely. The date of Agathon's death is, however, not certain if the scholium to *Frogs* 85 can be interpreted as referring to the death of Archelaos (400/399 B.C.) rather than the death of Agathon. Another piece of evidence is Aristophanes' *Gerytades*; Lévéque accepts the date 407 and does not notice Raubitschek's arguments for 400/390 (*RE.*, s.v. Philonides); if this date is right, the mockery of Agathon would have some

point if he had returned to Athens after Archelaos' death. This is all uncertain but ought to be considered; *embolima* in the early fourth century (not long if at all before the *Ecclesiazusae* and *Plutus*) seem more likely than *embolima* in the fifth, and a second Athenian residence would account for the considerable interest in Agathon shown by Plato and Aristotle.

T. B. L. WEBSTER.

**Marginalla Scaenica.** By J. JACKSON. Pp. ix + 250. London: Geoffrey Cumberlege, 1955. 30s.

Students of the Greek dramatists will be grateful indeed to the editors of the Oxford Classical Monographs for the inclusion in their series of this posthumous work of textual criticism. The outstanding quality of Jackson's articles in *CQ* 35 attracted less immediate attention than it deserved, the date being 1941; the substance of these, with some omissions, has been incorporated here with much additional material, and the whole has been admirably edited and indexed by Professor Eduard Fraenkel, who in a brief and rather moving preface describes the way in which this book came into being and the extraordinary handicaps under which all this lonely scholar's work was produced.

The passages discussed are divided into three chapters, under the head of transpositions, omissions and glosses, and miscellaneous, the last being often also treated under various groups—faults arising out of ducatus litterarum, obliteration of parenthesis, punctuation, attribution to speaker, etc. The argument is thus to some extent cumulative, or at least the juxtapositions are often enlightening. Yet in spite of the impressive list in Appendix C of errors curable by transposition of words, it may be doubted whether it is possible to establish any general principle like Porson's *Tutissima corrigendi ratio est vocularum, si opus est, transpositio*, or Jackson's own view 'that neither in trimeters nor in lyrics can anything patently abnormal, whether linguistically or metrically, be regarded as established, if it would vanish upon a readjustment of the *ordo verborum*'. It is dangerous, in matters of textual criticism, to seek to determine any priorities of treatment. Nevertheless, there can be nothing but gratitude for the countless apt parallels in types of error produced for illustration by Jackson out of his vast store of learning. These parallels are not confined to the drama (indeed, some of the brightest gems in the book are incidental emendations in Libanius, Lysias, Plutarch); they range from the most general to the most particular, and are unfailingly relevant and instructive.

It is, of course, impossible in a short review either to summarise or to discuss adequately the treatment of so many individual passages. Any reader will find himself constantly obliged to annotate his copies of the dramatists, especially of Euripides and Aristophanes. Some of these corrections are slight, simple, and immediately persuasive, like *E. Tro.* 809 *ἀνδρῶνες*, 1206 *καὶ δις αὐτὸς*, *Phoen.* 1279 spoken by Jocasta to the Messenger, *Bacch.* 1220 *ἐν ταύτῃ πύλῳ*, *Cyc.* 340 *τὴν ἐμὴν ψυχὴν δ'* and 343 *τὸνδε χαλκόν*, *Ar. Lys.* 238 spoken by Lysistrata, finishing her sentence, and 316 *πρὸ τῶνδ' ἐμοί*, *Ecl.* 509 *καὶ Μελῶν*, *S. Phil.* 686 *τὸς δ' αὖθις θαυμά μ' ἔχει* (why have we acquiesced so long in Dindorf's *τοί*?), 983 *στῆλιν ὁμοῦ τοιοῦδ'*. Among the most complicated are those depending on transposition of lines. It is a game which many critics have played, and only too often either the process of corruption is inexplicable or the result is to introduce new sources of dissatisfaction in the endeavour to eliminate existing ones. Jackson's manipulation of *Bacch.* 752 ff. is too elaborate to convince, but his *IT* 766 ff. is excellent, in spite of apparent complexity; at *Held.* 945 ff. he had been partly anticipated by Wilamowitz *GV* p. 544, but his modification *ἀγῶνας* 947 is a neat improvement. On *Held.* 685 ff. see now Zuntz, *The Political Plays of Euripides*, pp. 113 f. The diagnosis of the trouble in *L4* 650 ff. is undoubtedly correct, and 652 and 665 must be thrown out, not emended; but in his new version 666 does not follow ideally on 661, and it hardly seems necessary to do more than replace 664 after 651, her repetition *μυρῶν* after *μυρῶν* then provoking the *συνετὰ λόγους* of his comment. Again, in *HF* 869 ff. it seems enough to replace 860 (with Kirchhoff's *τάχος ἐμπροσθεν ὁμαρτῶν δ'*) after 870, deleting the stop so that the infinitives depend on *ἀνακαλῶν*, without the further emendations.

Even where we may not be prepared to agree with his remedy, there is hardly a passage where his analysis of the difficulties does not illuminate our understanding of the Greek. The whole is written with a highly characteristic dry wit which never lapses into facetiousness and an urbanity in controversy which is never less than courteous.

Professor Fraenkel asks indulgence for much that 'must still have slipped through the meshes' in the laborious and important task of seeing this book through the Press, but the mesh was a fine one. By a curious freak, of the very few slips discernible



two are in the passage from *IA* with which the book opens, ποθ' for ποθ' in 659, and the obtrusive γ' in 664 which is only a Byzantine 'correction'.

A. M. DALE.

# Herodotus. Livre IX (Calliope) and Index Analytique.

By PH. E. LEGRAND [Assn. G. Budé]. Pp. 109 and 247. Paris: Les Belles Lettres, 1954. Price not stated.

The final volume of the Budé Herodotus appears as the posthumous work of its editor, who did not live to revise and correct it. This has been done by M. André Plassart, so that we have not lost the benefit of Legrand's latest labours. As in previous volumes, there are short notes at the foot of the translation giving cross-references, parallel passages from other authors, and occasionally discussions of readings noted in the apparatus to the Greek text. There is one *Notice*, devoted to the pursuit of the Persian host to Asia as described in IX 90-122, where one of two interesting matters are treated.

C. 122, the last in our text, tells how Cyrus refused the suggestion of Artembares that after his triumphs he should now move the seat of rule from Persia to some richer territory. His reason was that in softer surroundings the Persian race would degenerate. Why was this anecdote tacked so flimsily on to the story of the rascally Artayctes, a descendant of Artembares, who was captured at the fall of Sestos and executed at the order of Xanthippus by nailing to a board?

Legrand asks whether Herodotus intended to continue his *History* beyond the fall of Sestos, at any rate when he wrote at the end of c. 121 κατά τὸ ἔτος τοῦτο οὐδὲν ἐν πλείον τοῦτων ἐγένετο, which suggests that more years are to be chronicled with their events. Though in fact other garrisons in Europe remained to be reduced after Sestos, he decides that no continuation was intended. Sestos was not only the place where Xerxes' bridge of boats touched the soil of Europe and therefore a fitting point to end the tale of his defeat, but also fell to Xanthippus, father of Pericles, in a campaign conducted by Athenians without Spartan help. The source of Herodotus for this account would be not only Athenian but Alcmaeonid, and would be particularly unfavourable to the behaviour of Leotychidas and the Spartans, unwilling at first to finish the war with the operations that culminated in the victory of Mycale, and ready to leave the Ionian Greeks still under Persian rule. For the actual crucifixion of Artayctes the half-barbarian people of Elaeus are left to bear the blame of instigators. To have gone further would have been to begin the less welcome story of Athenian imperialism. The unfinished appearance is due to lack of final revision, which would have removed the sentence quoted from c. 121, along with other promises of parts never actually written or at any rate never embodied in our text.

C. 122, on the other hand, Legrand is inclined to regard as the addition of another writer who wished to conclude the whole with a moral suggested by reading of the Hippocratic essay *Airs Waters Places*, which emphasises the effect of the physical environment on martial spirit. Certainly no attentive reader of *Airs Waters Places*, and not only of c. 24, can fail to be reminded of this chapter in Herodotus, but I still feel inclined to agree with How and Wells *ad loc.* who declare, 'It should never have been doubted that this anecdote was deliberately chosen by the historian to close his work. . . . No doubt the moral is a little obvious, the literary artifice somewhat naive, but is not all this characteristic of Herodotus?'.

E. D. PHILLIPS.

# Thucydides. La Guerre du Péloponnèse. Livre I.

Texte établi et traduit par J. DE ROMILLY. Pp. lix + 107. Paris: Société d'Édition 'Les Belles Lettres', 1953. Price not stated.

The first volume of the Budé edition of Thucydides is most welcome. It gives us an admirably judicious biography of Thucydides, an up-to-date description of the manuscript evidence (to which we must now add *PHamb* 163 and 164), a sober and scholarly text, and a translation which is not only lucid and accurate but conspicuously avoids the structural elaboration of the original.

The manuscript evidence on which Hude, Stuart Jones, and Powell based their editions has been thoroughly and profitably re-examined by Professor de Romilly and her collaborators. She has discarded the long-established division of the stemma into two 'families', CG/ABEFM, by relegating G—rightly, in my opinion—to the *recentiores* (pp. xxiv-xxv). Several MSS. frequently mentioned in Stahl's jumbled apparatus but disguised in the O.C.T. under the collective anonymity of 'recc.' have regained the dignity of sigla. Their value, however, will be more apparent in Books V-VIII than it is in Book I. They often betray the hand of a corrector concerned to tidy up the text which confronted him, e.g. 24. 3 δυνάμεις καὶ πόλεις K,

26. 4 ὥς δὲ . . . ὑπήκουσαν, στρατεύουσιν H, 35. 5 δὲ add. S<sup>2</sup>. In 80, 4 Professor de Romilly, following J, prints ἀλλὰ τοῖς χρήμασιν; ἀλλὰ πολλὰ πλεον ἐπὶ τοῖς (τούτων codd.) ἐλαίμοις and in 136. 4, following H, καὶ γὰρ αὖ ὅτ' ἐκείνου πολλὰ δασυέσσιον (-ου codd.) ἐν τῷ παρόντι κακῶς πάσχουσιν. In the latter passage I should prefer the δασυέσσιον of K<sup>2</sup>, but the primary MSS. are certainly wrong; in the former, comparison with vii 48, 6 μὴ χρήμασιν, ὧν (B; ὥς cett.) πολὺ κρείσσους εἶσι, νικηθέντας ἀπέναι may give us pause.

Professor de Romilly is not hospitable to conjectures. She keeps the MSS. readings at e.g. 35. 5 ('difficile et suspecte', but the footnote hints at the right interpretation), 37. 2 (an indefensible and very easily emended grammatical abnormality), 91. 1 τῶν δὲ ἄλλων καὶ (the footnote does not show awareness of the chief difficulty), 103. 1 δεκάτῃ ἐτει (see now Lewis in *Historia* 2, p. 415), 113. 4, 140. 5 (where αἱ refers to the authors of the *πειρα* just mentioned, and is readily intelligible), and 142. 3 (see below). The majority of the emendations adopted are of long standing and are widely accepted, e.g. 2. 6 μετακλήσεις Ullrich (μετοχὰς ἐς), 15. 2 περιεγένετο Tournier (παρεγένετο), 57. 6 τεσσάρων Krueger (δέσας; but the plausibility of τεσσάρων rather than Busolt's *δουο* rests solely on the hypothesis that at some stage in transmission numbers were indicated by acrophonic numerals), and 61. 4 ἐπὶ Στρέψαν Pluygers (ἐπιστρέψαντες). Herwerden's deletion of ἀπὸ Ταναῶρον in 128. 1 should not have been perpetuated; the words are needed to complete the explanation of which they form a part and grammatically are unobjectionable. Professor de Romilly herself emends διοχλίσαι to τριοχλίσαι in 29. 1, for conformity with τριοχλίσαι in 27. 2 (I should prefer to keep 29. 1 intact and emend 27. 2, where the immediately preceding τριάκοντα may well have caused the corruption of διοχλίσαι to τριοχλίσαι), transposes καὶ Κυπρίους in 112. 4 to follow καὶ Κίλιξ (why?), and suggests the transposition of καὶ τελευτάσθαι in 119 to precede καὶ τότε (the passage is unobjectionable, despite the perplexity which Steup induced in himself).

Twenty-three passages are discussed in supplementary notes (pp. 101-7). In a few of these (1. 2, 69. 2, 103. 1, 125. 2) difficulties raised in Gomme's commentary are ignored or lightly dismissed. The rest do not take us beyond Gomme, except 9. 3, a defence of καὶ . . . τε, despite the notorious uncertainty of the transmission of connecting particles in the text of Thucydides; 25. 4, a worthwhile elucidation with a good Herodotean parallel; 50. 1, on the reflexive αὐτῶν; and 76. 3, a defence of the subjunctive without ἐν in relative clauses, which prompts the reader to ask why, if Thucydides found this poetic usage acceptable at all in prose, he apparently did not use it more often. It would be unreasonable to demand in a Budé edition comment on a scale without which the translation of some passages, e.g. 77. 1, is bound to remain unsatisfying; but surely 142. 3 has a stronger claim than most to a supplementary note, and in 132. 2 the translation of ἴσως . . . τοῖς παροῦσι is not adequately explained by the footnote.

K. J. DOVER.

# Isokrates. Seine Anschauungen im Lichte seiner Schriften. (Annales Academiae Scientiarum Fennicae, Ser. B, 89.) By E. MIKKOLA. Pp. 347. Helsinki: Finnish Academy of Sciences, 1954. Price not stated.

Isocrates has always suffered through comparison with his illustrious contemporary Plato, and will no doubt continue to do so. Mikkola claims that the traditional view is unfair to Isocrates in other ways as well. Because he didn't always think differently from his predecessors he has been treated as a bee that flits from flower to flower culling the thoughts of others. Because he paid great attention to the form in which he expressed himself, the content of his work has been unduly depreciated even by those who have recognised his importance in the history of humanism. It is the aim of Mikkola's book to redress the balance by a thoroughgoing and systematic investigation of the thought of Isocrates as revealed in his writings. For this purpose his thought is brought under two main headings—Thoughts about Reality, with subdivisions Knowledge, Religion and Values, and Thoughts about the Structure of Experienced Reality, where the main subdivisions are into Human Action, Fellowship of Man, Logos, Education, and (very briefly) Political Views. In each case the method is to treat related thought-concepts, for example ἐπιστήμη, ἀλήθεια, εἶδος and alternative expressions for these, and to analyse the implications of their use in Isocrates' works with special attention to key passages. The aim is to reach the underlying scheme of thought. A useful feature of the work is tables of frequency for many of these words, unfortunately without proper comparative figures for other authors, and very full indices of words and passages in Isocrates.



The conclusion reached is that Isocrates' writings rest upon a basis of consistent and homogeneous thinking which sprang from some particular contentions within the sphere of the theory of knowledge. Because the acquisition of certain knowledge, of true *ἐπιστήμη*, is denied to men we are left only with the possibility of taking the world of phenomena both within us and without simply as it is, and this means taking it in all its multiplicity without referring it to any ultimate reality outside itself which might reduce its multiplicity to a more manageable compass. Probability must replace unobtainable certainty in the sphere of values as elsewhere, and it is the task of experience and individual feeling to construct the world of values with reference to this multiplicity. From this fundamental view of reality there follow the various practical conclusions which constitute the well-known Isocratean flavour of thought which pervades his writings.

If this brief summary does not too seriously misrepresent Mikkola's view of Isocrates, it will be clear that he regards Isocrates as essentially a systematic thinker. It is true that from time to time he states the contrary, for example, in discussing Isocrates' views about education, but this is regarded simply as making our task more difficult. There is no hesitancy in describing what this task is—to arrive at the reasoning which underlies Isocrates' unsystematic exposition of his ideas. It is true also that the reasoning is recognised to be wholeheartedly anti-metaphysical, and to involve the rejection even of the possibility of metaphysical knowledge. But this rejection itself is regarded as springing from a systematic view of man and the universe. And Mikkola seems, if I understand him rightly, to treat this view as one which was in the main developed by Isocrates himself.

Before we can accept this picture of Isocrates as entitled to be regarded as a philosopher in our sense of the word, there are many problems which need to be discussed, not all of which are sufficiently considered in the present book despite its ample scope. First and foremost must be the question how far a coherent system of reasoning underlies Isocrates' writings and what is the nature of its coherence. But hardly less important will be a second question—in so far as there is a coherent system, what is its source and what are its affinities with the work of other thinkers, and this second question would need much fuller treatment than it is given in the present book. Three points may be mentioned which are especially relevant to the first question. The rejection of *ἐπιστήμη*, whether it be in Plato's sense, or as is more probable in the sense appropriate to Parmenides, can be seen to lead naturally enough to a theory of probability and from a theory of probability to a doctrine of persuasion. But it is not so easy to see how such a starting point leads necessarily to the more particular moral and political doctrines which Isocrates wished to persuade his contemporaries to accept. Yet Mikkola is prepared to say, for example (p. 174), that it is the epistemological standpoint of Isocrates which led him to the doctrine of the fellowship of man, and which brought him much closer to his fellow men than Plato the intellectualist. Secondly, the attempt to uncover a latent system of thought, though often necessary, is inherently dangerous, as a system may be found of which the author was quite unaware. An example may be taken from the treatment of *Antidosis* 76–80. This is said to give the theory of values of Isocrates in *nuce*, treated both from a logical and a systematic point of view. We are invited to derive from it a table of values arranged in hierarchical order. But it is at least possible to read the passage in quite a different way. Is not Isocrates simply commending certain policies in which he happens to believe, in terms calculated to appeal to his readers because he knows that they share the moral feelings to which he refers? If this is even partly the case it is dangerous to attempt to derive a system of thought for Isocrates from the passage. This leads us to a third point. One might expect to find that a writer such as Isocrates would be coherent in the sense that he would not try to combine wholly incompatible ideas. But this would be consistent with a group of loosely held general themes, the logical interrelation of which was never submitted to close examination. Can we be sure that Isocrates attempted to carry coherence beyond this point?

In so far as Isocrates' ideas did spring from a coherent body of thought it will be pertinent to ask about its provenance. Jaeger regarded Isocrates as essentially continuing the educational system of the sophists and rhetors, and regarded the conflict between Plato and Isocrates as the first battle between philosophy and rhetoric. Mikkola is probably right in characterising this view of Isocrates as too negative. The conflict between Plato and Isocrates has been exaggerated, to say the least. The sophists were many of them philosophers as well as rhetoricians, and so in part at least was Isocrates. But we would like to know how far Isocrates is simply taking over sophistic ideas, especially those of Gorgias, and how far he is developing

ideas of his own. And there is another possibility that needs to be borne in mind. Especially in moral and religious matters, Isocrates may simply be acting as a mirror to the thought of the day, whether or not this in turn was the product of the fifth-century sophistic movement.

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**Plutarch. Moralia. Vol. VI, fasc. 1.** Ed. C. HUBERT. Pp. xxii + 194. Leipzig: Teubner, 1954. DM. 8.80.

**Plutarch. Moralia. Vol. VI, fasc. 2.** Ed. M. POHLENZ. Pp. xii + 224. Leipzig: Teubner, 1952.

**Plutarch. Moralia. Vol. VI, fasc. 3.** Ed. K. ZIEGLER and M. POHLENZ. Pp. viii + 50. Leipzig: Teubner, 1953. DM. 2.40.

**Animadversiones ad Plutarchi libellum περὶ εὐθymίας.** By H. BROECKER. Pp. 242. Bonn: Rudolf Habelt, 1954. DM. 7.80.

**Plutarque. Le Banquet des sages.** Texte et traduction avec une introduction et des notes par J. DEFRADES. Pp. 115. Paris: Klincksieck, 1954. Fr. 800.

In these works we are given an interesting *exposé* of Plutarchean scholarship, in which the main variety of fare lies in the difference of approach between the contributors. If the total impression is of a too richly seasoned meal, the responsibility lies with the Germans, whose erudition is thorough and often excessive. Throughout their work in this group the master hand of M. Pohlenz exerts the greatest influence.

Of the new Teubner Plutarch, the sixth volume, in three fascioli, continues the series begun in 1925, a series greatly needed to supersede the previous edition of Bernardakis. The previous volumes have been severally received with mixed feelings. Perhaps the most unfavourable criticisms were expressed in 1937 by W. C. Helmbold and F. C. Babbitt (*CP* XXXII, 1937, pp. 78–81), who pointed out that it is the prime duty of an editor to collate the MSS. and to report faithfully the divergences, rather than to fill the *apparatus criticus* with conjectures, however brilliant. This the earlier volumes, in the opinion of these reviewers, failed to do. Such censure was perhaps too strict in that it overlooked the good points of the new edition, but the same criticism can, with modifications, be made against portions of the sixth volume. Ziegler, for instance, editing the pseudo-Plutarchean *De Musica* in Fasc. 3, states on pp. vi–vii of the *Praefatio*: *sed quoniam talia* (sc. apparently derivative or related MSS.) *ad constituendum verum textum nihil fere pertineant, nolui apparatus criticum eius modi notis onerare*. This, at least, is honest, but as a general principle it is not to be commended. The sixth volume, like earlier volumes, provides a useful set of notes, placed directly above the critical apparatus, containing references to similar passages in other authors and to a select, but not exhaustive, number of modern discussions. One suspects that in some cases the editor's erudition and the attention which he has paid to parallel passages has caused him to neglect the MSS. and so led to his undoing. The comparative method has its disadvantages as well as its advantages.

Fasc. 1, edited by C. Hubert, contains *Aqua an ignis utilior*, *De sollertia animalium*, *Bruta ratione uti*, *De esu carnis* I & II, *Platonice Quaestiones*, *De Animae Procreatione in Timaeo*, and *Epitome libri de Animae Procreatione in Timaeo*. The edition is sober and in some respects the best fasciculus of the whole volume. Hubert has taken pains both in his discussion of the MSS. in the preface and in reporting their variants in the *apparatus criticus*. He has little new of his own and nothing spectacular to offer. He often seeks to defend the MSS. where other editors (in some cases unnecessarily) make alterations or insertions, e.g. 990B καλόμενος against βαλόμενος (Wilamowitz); 993 C-D, ὁρίσαντες εἰς ἡδονάς: (ἐξ)ὁρίσας. ci. Bern. ὁρίσας. (ἐκπεσόντες) dub. Po.: sed ὁρίσ. εἰς ἡδ. (libidinosis facti ad voluptates, cf. etiam p. 97, 10 εἰς ἀμώτητα) rectum esse vid. Hu.; 997 F καλοῦντα against (παρὰ)καλοῦντα (Kronenberg). Cf. 1001 A—'post πεποιθὲν iterat ὁ μὴ ἀνάπαυιν Mez. Ha. <ὁ μὴ δ' πεπονηκὼς γεγένηκεν> add. Po.; neutrum opus vid. Hu.'; also 1007 D, where additions are rejected, and 1026 C where the MSS. μὴν is retained. Less happy is the interpretation of the MSS. καὶ ἀναγίων in 1005 A—'rectum vid., si ἀναγίων sensu activo accipitur'—where the comparison with 337 A is misleading. The Greek language is rendered poorer by one word, παλιμμεταβολῆς, which is separated without ado into παλιν μεταβολῆς (998 D). The most important corrections accepted are those of other editors, those of Hubert himself being confined mainly to the *apparatus* (e.g. 995 A νεκρώδες for κρεώδες). A few of his suggestions are printed in the text. Though small, they are not without point—e.g. 1004 E τὴν λίθον for τὸν λίθον (to give consistency to a passage where the word is used several times in the feminine gender); 1013 E προεβντίαν for προεβύτερον:



1021 A <τὸ> add. Hu. One of Hubert's suggestions, put forward with hesitation—995 F *ἀν' ἐπαράξῃ* for *οὐ ἐπαράξῃ*—has the sole object of avoiding hiatus: yet at 974 D occurs the suggestion: <οἱ> *δὲ* *τὸν* dub. Hu. This defies the well-established practice of the Tübner *Moralia* of greeting hiatus in the MSS. or in the conjectures of other editors with the alarmed interjection—(hiatus!). Hubert, himself, however, knows that hiatus can be found in Plutarch: he enumerates four examples on p. 78. It would serve no useful purpose to look for misprints in a work of this nature, but one might at least expect the numbers of marginal pages to be correct. 1011 (p. 140) is wrongly printed as 1101.

Fasc. 2 contains *De Stoicorum repugnantibus, Stoicos absurdiora poetis dicere, De communibus notitiis contra Stoicos, Non posse suaviter vivi secundum Epicurum, Adversus Colotem et De latenter vivendo*, and is edited by M. Pohlenz. This scholar, whose knowledge of the Stoa and of later Greek thought is unquestioned, has already made his contribution to learning, and one might have assumed that he was the fittest person to undertake the editorship of Plutarch's works on the Stoics and Epicureans. In a sense this is true, but there is too strong a tendency to rewrite Plutarch in the light of what other authors wrote. Sometimes the practice is justifiable and produces good results: e.g. 1060 A *ὡς περ ἐπιβαδρῶν ἀναβαίνων* *δοκοῦσι* corr. Po. cf. Sext. VIII, 481 Epict. I 7, 22, 25, 29 Plut. Alc. 17 Demetr. 8 Plot. I 6, 1. Sometimes acquaintance with parallel passages leads to the intrusion of references in the apparatus (e.g. 1059 F *καὶ* cf. *commo* Cic. Tusc. IV 54) presumably to demonstrate how the MSS. can be defended should anyone wish to attack them. Often the comparative method leads to the alteration of a text intelligible in itself and otherwise unobjectionable, e.g. 1084 D *σπέρματος* for *σώματος* (cf. SVF II 805 et 741 sqq): 1085 A <ἐν> *σποκαμένως* (add. Po. ex SVF II, 89, 33). In general, there is excessive zeal for improving Plutarch in accordance with an arbitrary canon of symmetry and self-consistency, which produces in many passages something which Plutarch ought to have written but probably never did—e.g. 1045 D *ἀδελφός* (ex v. 4 et p. 32, 1) Po. (the comparison with p. 32, 1 is in any case unsafe, since there *ἀδελφός* is a conjecture of Wytenbach). The following: 1060 D <καὶ ἀσπίς>, 1069 B <... ἀπειροκαλόν>, 1109 C <ἀμφοτέρωσιν τοιούτων εἶναι>, 1120 E <καὶ φύγεσθαι καὶ θεωρεῖσθαι>, 1122 D <... βωδόντων> are gratuitous additions arising out of little more than a desire for symmetry in expression. Where Plutarch deals with pairs of opposites, as in 1120 E, it would have been odd to name one member of a pair without the other (hence here <καὶ φωτεινὸς ὁ ἥλιος> is justified), but he did not necessarily give a full list of pairs each time he spoke of the opposites. This much is clear from his practice in 1110 A ff. A similar lack (or was it avoidance?) of absolute symmetry would account for the omission of *ἐν* *ἐν* *στρατηγεῖν*, which one might expect in 1001 E (Fasc. 1, p. 119, 4), where Hubert treads more carefully and speaks more wisely than one imagines Pohlenz would have done. Among other unnecessary insertions in Fasc. 2 are 1094 D <ποταμός>: 1104 A <παρέρχων>. A number of alterations are perhaps overconfidently regarded as corrections—e.g. 1041 B *διαφορά* for MSS. *διασφορά*. Plato, of course, in the passages to which Pohlenz refers, does speak of injustice as *σάσις* and *διασφορά* in the soul, but Plutarch is not quoting Plato directly at this point, and elsewhere Plato implies the 'destruction' of the soul by injustice (*διαφθορά*—Rep. 445 A). The exclamation point is well worked in the *apparatus criticus* to herald the appearance of hiatus (in 1114 B Pohlenz himself is saved from hiatus in his reading *μύνηται*, *ἀς* ... by a comma which is not essential) and once it expresses a new range of editorial emotion, viz.: 1061 C *ἐν τοῖς ἐργοῖς* <τοῖς> *δὲ* *τοῖς*—*τοῖς* haplogr.!) add. Po.

Ziegler's hesitation in accepting the editorship of *De Musica*, the first work in Fasc. 3 (cf. *Praefatio* p. 1), is understandable in view of the difficulties involved (see also next review). Although he has done much good for Plutarch elsewhere, one feels that he was not at home in the present task. His explanation of one feature of the *apparatus* has been quoted above. The result is that the *apparatus* becomes to a large extent a compendium of conjectures, among which his own play a small and unimportant part. The rest of Fasc. 3 contains the pseudo-Plutarchean *De libidine et aegritudine* and *Parasceue an facultas animi sit vita passio*, both edited by Pohlenz. The editorial standard and achievements are naturally much like those of Fasc. 2.

*Animadversiones ad Plutarchi libellum de libidine et aegritudine* is a promising work by a young German scholar which uses the comparative method to advantage. Broecker's object is to examine the relationship between Plutarch in the *De tranquillitate animi* and his sources, and to discover the original which he mainly followed. No attempt is made to edit the text—*nam editio Pohlenzi tam accurata est, ut nullus fere locus emendatione egeat* (p. 17). This surprising declaration of faith finds its corollary in the adoption of the Pohlenz pagination in the Teubner

edition, which, despite a concordance with the traditional Stephanus pages, is an inconvenience for anyone who should wish to lay Pohlenz momentarily aside! In analysing the *sententiae* of Plutarch, however, Broecker shows a happy independence. His analyses, which are almost scientific in their thoroughness and their care for *minutiae*, display a sound knowledge of Plutarch and of Stoic writers. The tables of comparisons which he draws up to demonstrate the use of similar *sententiae* and even of identical phraseology by Plutarch elsewhere and by other writers are always valuable—and sometimes precious. By methods such as these Broecker is able to relate Plutarch's essay to the similar essay of Seneca and to certain passages in Cicero, and the conclusion which emerges clearly and convincingly is that Plutarch's main source was the *πρὸς εὐθυμίας* of Panaitios; Broecker is further able to sketch the contents of Panaitios' lost work with the aid of a complex diagram on p. 203. On his thoroughness, even if it is laborious, and his clarity, Broecker must be heartily congratulated. Perhaps his chief fault is his failure to discuss the relative chronology of Plutarch's moral essays; this should be a first necessity for one who compares so much and so closely, and it would have improved the logic of the argument—e.g. on p. 150. A clue at 467 E (τοὺς Ῥωμαίων δὲ βασιλεῖς, ὧν οὐδεὶς οὐδὲν τὴν ἀρχὴν ἀπέχετο), together with a consideration of the *De Iside et Osiride* (cf. Ziegler, *Plutarchos von Chaironeia* (1949), 80), might have affected the interpretation offered there. In addition to his main thesis, Broecker throws interesting light on Plutarch's method of working in such essays as the *De tranquillitate animi*. He also clearly has enough material of the right sort to enable him to draw conclusions on such topics as the paralytic effect of commonplaces on literary invention in the Graeco-Roman world. Unfortunately, he adheres closely to his theme and does not allow himself the liberty of such digressions. Consequently the book remains a specialised study which will appeal only to specialists. Broecker's Latin is usually sound, but not when he writes (p. 202): '*Plutarchus suo arbitrio adiunxisse argumentum de metempsychosin et de aeternitate...*' *supra* pp. 180/1 *dissertat*.

The French contribution to the banquet is its most easily digestible course. Defradas is no pedant, but succeeds in being reasonably thorough without being laborious. His introduction deals intelligently with a number of questions—the problem of authenticity, the composition of the work, the characters of the Seven Sages and others who appear at the Banquet, the philosophic content, the sources, the date, and the manuscript tradition. He agrees in the main with Ziegler in regarding the *Banquet* as an authentic work of Plutarch's mature years, and supports this view with a consideration of certain Platonic elements which it embodies: 'Cette inspiration platonicienne nous paraît être la meilleure preuve de l'authenticité du *Banquet*' (p. 15). The translation is based on the Teubner text of Paton and Weghaupt, but without slavish adherence. It is diffuse and readable and at the same time renders the Greek with commendable accuracy. The notes are interesting and helpful. One would have liked perhaps to be told more about the popularity of the legends of the seven sages, Aesop and Arion in Plutarch's own day (e.g. by a comparison with Dio Chrysostom, xxxvii, 2 ff.), but on the whole the introduction and commentary are well balanced without any pretence of being exhaustive. Defradas is not particularly happy in some of his textual emendations, especially 156 E, where his note (p. 106, note 136) betrays his uneasiness: 'En réalité, on doit imaginer ici une lacune de plusieurs lignes'—and one might reasonably expect references to a later edition of Diels, *Fragmente der Vorsokratiker* than the fourth (e.g. p. 106 n. 141; p. 107 n. 151). Incidentally the index to Dittenberger's *Sylloge* is not the ideal place for discovering the frequency of the proper names Diokles and Nikarchos (cf. p. 16).

A. J. GOSSAGE.

**Plutarch, de musica.** Ed. K. ZIEGLER (in *Plutarchus Moralia VI 3*. Bibliotheca Teubneriana. Ed. K. ZIEGLER and M. POHLENZ. Leipzig, 1953) (see also preceding review).

**Plutarque de la musique.** Texte, traduction, commentaire précédés d'une étude sur l'éducation musicale dans la Grèce antique. By FRANÇOIS LASSERRE. Pp. 185. Olten & Lausanne: Urs Graf-Verlag, 1954. (Bibliotheca Helvetica Romana, I.) £2 4s.

It seems ungrateful to criticise Weil and Reinach, but so capriciously did they deal with the text of the *de musica* that, even if Wilamowitz (*Griechische Verskunst*, 76, n. 3) was too harsh, the need for a new edition has long been felt. Now, more than thirty years after he wrote, two texts appear almost simultaneously.

Ziegler, who took over the work from Sieveking (after much



hesitation, as he tells us), lists the MSS. of the two traditions, Plutarchean and musical; he remarks, justly enough on the whole, that, apart from their own errors, neither the individual MSS. nor the two groups differ very significantly from one another; after some perfunctory observations about their relationships, he announces his intention of quoting few variant readings but including in his apparatus a full account of the conjectures of modern scholars. This is useful in its way, but hardly what we expect of a new Teubner. On this basis, however, he has produced a sensible and serviceable text, choosing judiciously among the conjectures of others and making some good suggestions himself. The following may be mentioned: p. 11, 21  $\sigma\acute{\upsilon}$  γὰρ for  $\sigma\acute{\upsilon}\tau\epsilon$  γὰρ; 13, 2  $\alpha\acute{\upsilon}\tau\eta\eta$  for  $\alpha\acute{\upsilon}\tau\eta\sigma$ ; 28, 26  $\epsilon\pi\sigma\theta\eta\mu\acute{\omega}\nu$  add.; 33, 25  $\delta\upsilon$  add. These are, of course, minor improvements; the major problems may be insoluble, and Z. does not hesitate to use obelus and asterisk. This is an eminently useful text to be going on with, but a bolder approach may some day lead to a more signal advance. The problem of dislocations cannot be shelved because Weil and Reinach committed excesses.

The virtues and defects of Ziegler and Lasserre are curiously complementary. L. has gone fairly thoroughly into the MSS., produces a stemma codicum, and prints an apparatus which can only be criticised on two grounds: an occasional excess of detail (e.g.  $\gamma\gamma\upsilon$ - and  $\gamma\upsilon\upsilon$ -) and a rather unhandy set of sigla (taken over in part from Ziegler and others).<sup>2</sup> Apart from the MSS. of independent authority, whose readings are fully reported, the stemma has to be taken on trust. To some extent the affiliations are confirmed by Düring's stemmata for Ptolemy and Porphyry<sup>3</sup> (and by my own investigations into the MSS. of Aristides Quintilianus). Occasional differences need not throw doubt upon L.'s account, but fuller information in the introduction would have ensured that this work need not be done again, when someone comes—as it is to be hoped he will—to a full-scale palaeographical study of the *musici scriptores*. L. finds that all the MSS., including Ambrosianus 859 (the MS. of Planudes on which the whole Plutarchean group depends), belong to one main branch of the musical tradition. His most striking conclusion is that the common readings (his  $\eta$ ) of Neap. III C 3 and Laur. 59.1 (a mixed MS. which also contains works of Plato) constitute an independent and often superior representative of the archetype.  $\eta$  certainly gives some good readings, e.g. 119, 6 (Lasserre's pages)  $\gamma\eta\mu\acute{\omega}\nu$ ; 121, 17  $\upsilon\pi\acute{\alpha}\tau\eta\varsigma$ ; 121, 18–19  $\tau\epsilon\tau\tau\acute{\alpha}\rho\omega\upsilon$  . . .  $\delta\iota\alpha$  (omitted by the other MSS.); 123, 10  $\pi\acute{\alpha}\lambda\eta\nu$ ; 127, 8  $\mu\acute{\epsilon}\lambda\omega\varsigma$ ; 127, 31  $\eta$   $\kappa\alpha\theta\acute{\alpha}\tau\eta\rho$   $\tau\iota\varsigma$   $\phi\alpha\sigma\iota\nu$ ; 131, 9  $\alpha\acute{\upsilon}\tau\eta\varsigma$ . Of these some might be, but it is unlikely that all are, the work of a learned corrector.

The text that L. constructs upon this sound foundation is marred by an obstinate conservatism, which leads him to reject plausible or even necessary conjectures. Here are a few examples. 112, 36  $\langle\sigma\acute{\upsilon}\rangle$   $\mu\alpha\lambda\lambda\acute{\omega}$   $\chi\rho\acute{\omicron}\nu\omega$  restores consistency to the passage at small expense. 114, 9  $\epsilon\pi\acute{\iota}$   $\mu\acute{\omicron}\nu\omicron\upsilon\varsigma$   $\tau\acute{\omicron}\upsilon\varsigma$   $\alpha\lambda\lambda\eta\lambda\acute{\omicron}\kappa\omicron\upsilon\varsigma$  is hardly defensible ( $\epsilon\pi\acute{\iota}$   $\mu\acute{\omicron}\nu\omicron\upsilon\varsigma$   $\tau\acute{\omicron}\upsilon\varsigma$   $\alpha\lambda\lambda\eta\lambda\acute{\omicron}\kappa\omicron\upsilon\varsigma$  Ziegler after Volkmann). 115, 21–22  $\epsilon\lambda$  . . . [8] restores intelligibility. 117, 21  $\mu\upsilon\sigma\iota\kappa\acute{\iota}\eta$  is obviously necessary. At 119, 8  $\eta\theta\omega\varsigma$   $\kappa\alpha\lambda\lambda\acute{\omicron}\varsigma$  and at 11  $\kappa\alpha\tau\acute{\alpha}$  (for  $\pi\rho\acute{\omicron}\varsigma$ ) should be accepted. At 119, 16–17 the text printed is not Greek. At 124, 18  $\epsilon\kappa\alpha\sigma\tau\omega\upsilon$  is a misprint for  $\epsilon\kappa\alpha\sigma\tau\omega$ , but  $\delta\eta\lambda\omicron\iota$  is self-evident. 124, 21–22  $\tau\eta\eta$   $\mu\upsilon\sigma\iota\kappa\acute{\iota}\eta\eta$   $\mu\upsilon\sigma\iota\kappa\acute{\iota}\eta\eta$  gives an impossible word-order. 126, 32  $\tau\alpha\upsilon\tau\eta$ , 127, 4  $\eta$   $\delta\eta\mu\omicron\kappa\iota\kappa\acute{\iota}\eta$   $\mu\upsilon\sigma\iota\kappa\acute{\iota}\eta$ , 129, 7  $\alpha\acute{\upsilon}\tau\omega\upsilon$  are as easy as they are plainly necessary. L. makes a dozen conjectures of his own, some of which deserve consideration. For  $\kappa\eta\delta\epsilon\sigma\iota\varsigma$  (112, 34) he argues plausibly on p. 106, but  $\langle\mu\upsilon\sigma\iota\kappa\acute{\iota}\omega\rangle$  (119, 26) is certainly wrong.

I have little space left in which to deal with L.'s notes and introduction. The notes are not intended to provide a full commentary in replacement of Weil and Reinach. They contain a useful set of references and parallel passages and discuss a number of special problems, sometimes too briefly. The introductory essay reviews the whole course of Greek music up to the end of the classical period, with especial reference to the place of music in education. It is focused upon the personality and work of Damon and includes as an appendix to Chapter VI a collection of 'fragments de l'aréopagitique'. L. believes that this was a genuine address to the Areopagus and that it, or its influence, can be traced not only in the passages which refer to Damon by name (collected by Wilamowitz and Diels), but also in Herodotus (6. 129) and Isocrates, *Areopagiticus*. Not everyone will be able to follow him in his confident belief, nor in the account which he gives of the work of Lasus of Hermione

(to whom he ascribes the ancient scales recorded by Aristides).<sup>4</sup> Admittedly, the evidence is scanty and confused, and any scholar setting out to write about the history of music during this period has to make bricks with very little straw. One can admire L.'s courage, while questioning his judgement at many points. He can, for instance, say (p. 17, of Homer and Hesiod): 'ainsi, témoignages positifs et témoignages négatifs s'accordent à montrer que la musique véritable n'entre pas dans la culture aristocratique et convient seulement, aux yeux des poètes épiques ou didactiques, au divertissement du peuple laborieux',<sup>5</sup> and (p. 20, of the Lesbian poets): 'il ressort de l'observation d'Aristoxène selon laquelle Alcée et Sapho ont eu "leurs livres pour confidentes" que les deux poètes accordaient encore au langage seul le privilège d'exprimer leurs sentiments'. But Solon, by contrast, is made into a forerunner of Damon through a forced interpretation of fr. 2, 2.

L.'s introductory essay deserves and will receive study; and he has made a big step forward towards a soundly based text of the *de musica*. But he cannot be said to have produced the edition of the treatise which is so badly needed.

R. P. WINNINGTON-INGRAM.

**Papyrus Bodmer I. Iliad chants 5 and 6.** Ed. V. MARTIN. Pp. 90, with 6 plates. Cologne-Geneva: Bibliotheca Bodmeriana, 1954. Sw.fr. 10.

In 1953 M. Bodmer acquired for his magnificent library near Geneva two papyrus rolls, one contained inside the other, which proved to contain the greater part of Iliad V and VI. M. Victor Martin publishes their text and discusses their importance with a thoroughness, lucidity, and charm that matches the elegance of this beautifully produced booklet (the third in the series *Bibliotheca Bodmeriana*). The Homer is written on the verso of a land-register, but is none the less a 'book' text. My instinct is to date it earlier than M. Martin does, and I believe it to be not later than A.D. 250. For the hand cf. P. Vat. Grec. 11 (Favorinus), P.S.I. 921 and P. Oxy. 654. It is to be regretted that none of the photographs is of facsimile size, and the data for calculating the amount of reduction have to be priced out of pp. 10 and 18. The scribe used a good exemplar, and the following sample readings may be given: 5.808 is omitted (with Aristarchus, and another papyrus, Pack No. 588); 5.785 in its second half offers an apparently unexampled formula  $\Sigma\tau\acute{\iota}\nu\tau\omicron\pi\iota$   $\epsilon\lambda\iota\sigma\mu\acute{\epsilon}\nu\eta$   $\eta\mu\acute{\iota}\nu$   $\delta\epsilon\mu\alpha$   $\eta\delta\epsilon$   $\kappa\alpha\iota$   $\alpha\acute{\upsilon}\delta\eta\eta\eta$ ; 5.486 uncontracted  $\delta\alpha\mu\epsilon\sigma\sigma\iota$ , in virtue of which Martin corrects Homeric Hymn to Hermes 58 to  $\delta\upsilon$   $\delta\alpha\rho\omega\upsilon$   $\delta\alpha\mu\epsilon\sigma\sigma\iota$ ; 5.603 it is claimed that an original second  $\alpha$  in  $\mu\alpha\rho\alpha$  has been altered to  $\epsilon$ , so that the papyrus reading is  $\tau\acute{\omega}$   $\delta'$   $\alpha\iota\epsilon\iota$   $\mu\alpha\rho'$   $\epsilon\upsilon\varsigma$   $\gamma\alpha$   $\theta\epsilon\omega\upsilon$ . If this is correct (to judge from the inadequate photograph it is possible to doubt whether the correction was not the other way, from  $\mu\alpha\rho\epsilon$  to  $\mu\alpha\rho\alpha$ ), Nauck's conjecture  $\epsilon\upsilon\varsigma$  for this line wins authority.

E. G. TURNER.

**Oxyrhynchus Papyri. Part XXII.** Edited with translations and notes by E. LOBEL and C. H. ROBERTS. Pp. xiii + 181, with 11 plates. London: Egypt Exploration Society, 1954. £5.

The latest volume is about equally divided between fragments of Ionic poetry (2309–28) edited by Dr. Lobel and a variety of literary and documentary texts (2329–53) edited (all in full) by Mr. Roberts. The first of Lobel's texts, 2309, consists of twenty-one incomplete lines of the Homeric *Margites*, firmly identified by its irregular variations of metre and its content. Among the remaining fragments Archilochus is prominent; to him belongs the extensive but puzzling 2310, in iambic trimeters, identified by the occurrence in it of a known fragment; 2311, containing mostly the beginnings of lines, is also probably from the same author's trimeters ( $\omega\gamma\lambda\alpha$  in Fr. 1 (a) 7 probably addresses one Glaucus, known from extant fragments); under 2312 are twenty-seven fragments, mostly small, similarly attributed on the strength of the very probable identification of two fragmentary lines with known lines of Archilochus, and the occurrence of another proper name (Lycambes); 2313 has thirty-eight fragments of trochaic tetrameters, one certainly identified with Archilochus Fr. 74, and two others with quotations from him in the Parian Monument; 2314, trochaic tetrameters, attribution probable; 2315–16, Epodes (*The Fox and the Eagle*), identification certain; 2317, trochaic tetrameters, probably Archilochus; 2318–20, Ionic fragments, mostly small and all anonymous. 2321 is

<sup>4</sup> When L. deals with scales, I find him unconvincing and not always easy to understand.

<sup>5</sup> L. can reach this conclusion by refusing to admit (e.g.) that the pleasure of Achilles at *Iliad* 9. 189 was derived in any significant degree from the melody (p. 13).

<sup>1</sup> Unless  $\sigma\acute{\upsilon}\tau\epsilon$  γὰρ is to be preferred?

<sup>2</sup>  $\alpha$  (Ambros. 859) and  $\epsilon$  (Laur. 59. 1) are particularly confusing in the types used.

<sup>3</sup> It is not quite clear to what extent L.'s classification actually depends upon these stemmata.



shown by subject and style and two probable identifications to be Anacreon; the attribution to the same poet of 2322 is less certain but very probable. The authorship of the small fragment 2323 is proved by its correspondence with a tiny scrap, Fr. 27, among the fragments of the Hipponax papyrus 2174 (in P. Oxy. XVIII); it also mentions Bupalus, familiar from his poems. 2324-6 are small anonymous fragments; 2327 has fragments of elegiacs of uncertain authorship. Dr. Lobel's work here shows his characteristic and unusual combination of brilliance, patience, and scholarly caution. The acumen which finds the connection of sense in mutilated passages never trespasses a hair's breadth beyond certainty in restoration or identification; brilliantly discovered probabilities are never presented as anything more.

2329, the first of the new literary texts edited by Roberts, is from an unidentified play of the New Comedy. According to the editor's convincing reconstruction, it gives us the end of one dialogue followed by another between a young man and his mother, and finally a soliloquy by a slave. 2330 is an almost completely preserved column of twenty-eight lines from the *Persica* of Ctesias; it is part of his romantic story of Zarinaea, Queen of the Sacae, and Stryangaeus the Mede, and quotes the letter of the latter to Zarinaea. The sentimental style is an extraordinary anticipation of that of the later Greek romances. 2331 has some crude verses on the Labours of Heracles, illuminated with a running series of coloured illustrations; their historical importance is discussed in a long note by Professor Weitzmann. 2332 is a long but almost incredibly corrupt and misappetizing third-century text of that fascinating but baffling Egyptian nationalist composition in Greek, the *Potter's Oracle*. It corresponds partly with one of the large fragments of the work already extant; its wide variations from the text of the latter were to be expected in popular literature of this kind. The editor's admirable commentary rightly emphasises the purely Egyptian inspiration and sympathies of the work, noting its allusion (l. 34) to King Bocchoris and the prophetic lamb, and its hostility to the Greeks (l. 33), which makes the 'city by the sea shore', previously known from this work and variously discussed, certainly Alexandria. I am less convinced by the editor's arguments for the early composition of the work, and particularly by its suggested connection with the Dublin papyrus of the third century B.C. cited on p. 92. After five texts (2333-7) of extant tragedies, of which one, 2336 (first century B.C.), the only papyrus of the *Helena*, and unfortunately very fragmentary, had a very different text from that known to us, we have in 2338 a document recording grants of *ἀνδρα* to the winners of the provincial Egyptian equivalents of the local *Eisteddfod* between A.D. 261 and 289; then (2339) a fragment which I think should certainly, in spite of some difficulties, be included among the *Acta Alexandrinorum*; note especially the mention of a petition *κατὰ τὴν ἀποδείξιν*, and compare the mention in PSI 1160 (the 'Boulé Papyrus', Musurillo, *Acts of the Pagan Martyrs*, No. 1, first century A.D., like the present text) of *ἀδελφοὶ καὶ ἀνέγνωσι γερουσίᾳ ἀνδράσιν* who would 'corrupt the pure citizen body of Alexandria'; in both cases I see a covert and spiteful reference to the Jews, who claim the advantages of Alexandrian citizenship without forgoing their non-Greek way of life. 2340 gives us a glimpse of industrial life and administration in Alexandria, while 2341 (on the other side of the same papyrus) shows us a prytanis of Oxyrhynchus accusing the strategus before the Prefect Subatianus Aquila of inefficient management of the corn-delivery, with a tantalisingly obscure description of the system by which the granaries were cleared for transport. 2342 (A.D. 102), a petition, has a use of *ἐνός* parallel to that postulated by the editor in Luke xvii, 21. 2344 (A.D. 336) mentions a 'bishop of the Catholic Church'. 2348 (A.D. 224) is interesting for students of testamentary law as showing one of the new citizens created by the *Constitutio Antoniniana* drawing up a Latin will *per aes et libram*. The rest of the documents are of more interest to the papyrologist than to the general reader. The standard of editorship is uniformly excellent.

JOHN BARRIS.

**Troy. The Third, Fourth and Fifth Settlements. Vol. II. Text and Plates.** By C. W. BLEGEN, J. L. CASKEY, M. RAWSON, and J. SPERLING. Pp. xxii + 325, with 318 plates. Princeton: University Press, 1951 (London: Geoffrey Cumberlege). 2354.

**Troy. The Sixth Settlement. Vol. III. Text and Plates.** By C. W. BLEGEN, J. L. CASKEY, and M. RAWSON. Pp. xxix + 418, and 512 plates. Princeton: University Press, 1953 (London: Geoffrey Cumberlege). 2388.

**Troy: the Human Remains (Supplementary Monograph I).** By J. L. ANGEL. Pp. 40, with 8 tables and 14 plates. Princeton: Princeton University Press, for University of Cincinnati, 1951. \$7.50.

The publication of the Cincinnati excavations at Troy marches steadily onwards. Since the first volume was reviewed in this *Journal* two more stately volumes have appeared. Professor Blegen and his collaborators are much to be congratulated not only on the excellent form in which they present their discoveries and conclusions, but on the pace which they are able to maintain. These two new volumes are in format and style similar to their predecessor and worthy of the reputation of the Princeton University Press. The photography, set up, plans, and illustrations are all good and do full justice to the photographs and drawings provided by the excavators. They are models of the manner in which excavation reports should be presented. Like the first volume, each of these two is in two parts; one contains the text, the other the illustrations. As in the first volume the authors make their report as factual as possible. Their aim is to describe what they found, and how they found it, clearly and impartially with all relevant detail. The site, with all its important stratification, has been transferred to paper, and so we hope preserved forever. Their interpretations and suggestions are carefully distinguished from the record of facts and observations. Every excavator knows, or should know, that how and where an object is found is really more important than the object itself. Readers of these volumes, too, must remember that what is reported in them replaces the preliminary reports, and that the stratigraphical observations here recorded confirm, amplify, and correct those of Schliemann and Dörpfeld. The excavators, too, on several occasions found that later digging caused them to revise their first notes and opinions. It is essential to keep all this in mind, for some readers who wish to synthesise or to extract evidence to support plausible theories have already been led into error and have confused strata. In this connection the footnote on p. 4 of Volume II is a most valuable illustration. All users of the volumes will do well to study it.

Volume II describes Troy III, IV, and V, settlements that have often been passed over and given too little attention. These three settlements were undeservedly described by Dörpfeld as poor or humble villages. They suffered much in Schliemann's excavations, for he cleared away a large part of them, especially of Troy III and V, in his desire to lay bare as large as possible an area of Troy II, the Burnt City, which he believed to be the Homeric Troy of Priam. The Cincinnati excavations have redeemed the reputation of these three settlements. Although not so important or so wealthy as Troy II, they are valuable archaeologically and historically from the cultural standpoint. It is not clear whether Troy III and IV were walled, but as noted long ago, they spread beyond the area occupied by the walled royal stronghold of Troy II. Troy V may have been walled, but no certainty is obtainable. As a result of the earlier excavations it was impossible to sort out the finds, both of pottery and other materials and of architecture, into a definite stratigraphic series, although Dörpfeld made some excavations with this object. Hubert Schmidt abandoned any attempt to distinguish the pottery of Settlements II-IV. It is therefore one of the great services of the Cincinnati expedition to have recorded the stratification of the pottery of these settlements, so that it can now be sorted out. We can now distinguish, for instance, the pottery of Troy IV. The importance of this for the observation of the evolution of culture at Troy is obvious. Further, the recognition of the pottery characteristic of Troy V, for instance, enables us now to be more sure in our attribution of the architectural remains of that settlement. A great advance in our knowledge of the history and development of Troy has been achieved.

Characteristic of Troy III are the so-called 'owl-faced' vases. The 'Depas Amphikypellon' is also of frequent occurrence. In plan the architecture is similar to that of Troy II, but in construction Troy III differs from the final phase of Troy II, II g. The exterior house walls of Troy III were generally built entirely of stone, and in some cases still stand to a height of 2 metres, whereas the earlier walls were usually of crude brick on a stone foundation. There was considerable accumulation of debris on the floors, which were in time covered with new earth and so the level gradually rose. Then the walls and roofs had to be raised too and the stone walls strengthened. Still, though there were innovations and a general reorientation of buildings and streets, this settlement carried on the traditions it inherited, and is demonstrably in general culture the natural and logical successor of Troy II. Features to note are the great prevalence of deer bones in the debris, the use of copper, and the considerable number of Early Aegean

<sup>1</sup> The attribution of the fragment is made certain by a citation from it by Demetrius, *De Elocutione*.



wares found. At least fifty sherds are Early Helladic, others are Early Cycladic. The comparative dating so far obtainable is only relative, but in general Troy III seems parallel to a middle stage of Early Helladic. The external connections are more with the West, but some with Central Anatolia are perhaps also discernible. Since the debris was about 3 metres thick and divides into at least three strata, we can assume that the settlement covered a minimum of three generations and would have existed about a century.

Dörpfeld always referred to Troy IV as a 'village' or a poor settlement. Schliemann was under the mistaken impression that the area of this settlement was no greater than that of Troy II. It is, however, considerably larger, and it may perhaps have been walled. The architecture continues the tradition of the two previous settlements, but whereas in Troy III the outside walls were of stone and crude brick was employed mainly for interior walls, this settlement reverts to the practice of Troy II. Domed ovens seem to be a new feature in this period, and suggest that in cooking at least the Trojans of that date made definite progress. Whether this type of oven was invented independently at Troy or was borrowed from abroad cannot yet be determined. It was known on the Mainland of Greece at an earlier date from Neolithic times onwards. The pottery continues and develops the tradition of Troy III, but the potter's wheel seems to have been more often employed. It never, however, reaches the quality of that of Troy V. As in Troy III a great number of deer bones and horns were found, indicating that the Trojans were a hunting folk. In general the external relations of Troy IV are with the West rather than with the North, East, or South. There were no doubt connections with Central Anatolia and the Near East, but the wares of the Early Bronze Age of the Aegean found at Troy show that it looked mainly westwards. The character of these wares suggests that Troy IV was broadly parallel to the penultimate phase of Early Helladic. The debris is about 2 metres in depth, and the excavators observed five stratigraphic phases indicated by architectural remains marking successive stages of occupation. If each phase is one generation, then we could assume a life of about 150 years for Troy IV.

Schliemann removed the greater part of the remains of the fifth settlement at Troy. He noted, however, that its area spread far beyond that of Troy II. Dörpfeld later attempted to unravel some of the complications of this layer, but he dismissed the architectural remains with the statement that they were like those of other 'humble villages'. It probably had walls or defensive works of some kind, though the later constructions of Troy VI seem to have replaced them when those great fortifications were erected. Troy V was probably of the same extent as Troy IV and occupied an area of about 18,000 square metres, almost as much as Troy VI.

This fifth settlement in the main continued the traditions of the immediately preceding settlements, and there was obviously no break in the development of culture at Troy. Certain improvements are to be noted in architecture, for though the materials, stone and brick, were the same, they were used in a more orderly manner and the houses were more spacious. The domed ovens and the hearths were better made. The floors were more free from litter, and this may indicate better housekeeping. The bones of deer decrease sharply, but those of oxen and swine increase greatly. Does this mark a greater dependence on domesticated as opposed to wild meat? Real bronze was now definitely in use. More conspicuous are the changes in the pottery, which displays better fabric and more refinement in shape. Imported Aegean wares are rare. The total deposit of this settlement is about 1.50 metres thick and is divided into four strata, and it follows on that of the fourth settlement without any interval. Troy V seems to have endured for about a century or perhaps somewhat more. Although as already stated fragments of imported Aegean pottery were rare, there are in their shape and decoration several indications of connections both with the Cyclades and with the Greek Mainland. Even the 'red cross' bowls have a general affinity with some of the later ware of the Early Helladic period. Relationship with the East, for instance Kusura, can also be discerned, but connections with Central Anatolia are less obvious. There are also indications of relations with Cyprus, presumably by coasting vessels rather than overland by way of Cilicia.

From these and other considerations Troy V seems to have been contemporaneous with the last phases of the Early Helladic period. We can thus infer that Troy V perished in the same disturbance which closed the Early Helladic period and initiated the Middle Helladic period on the Greek Mainland. This movement of peoples seems to have coincided with a corresponding shift of population in Central Anatolia in which the first Hittites dominated an older Bronze Age culture. This simultaneous movement in these three regions, Central Anatolia,

Western Anatolia, the Aegean, closes the early Bronze Age and ushers in the Middle Bronze Age, a stage of the utmost importance in the history of man in the Near East.

Schliemann never knew the sixth settlement at Troy. He not unnaturally concentrated his work on the central part of the mound, but there the building of the pretentious Athena temple had levelled off everything above the strata of Troy V. He had encountered the great fortification wall of Troy VI, but the very regularity and excellence of its construction misled him. Only in his final campaign in 1890, when he dug well outside the area of Troy II to the south-west, did he find foundations of large buildings associated not only with Grey Minyan ware (which he called Lydian), but also with Mycenaean pottery familiar to him from the Homeric citadels of Tiryns and Mycenae. He intended to investigate this the following year, but his death in December 1890 left the task to his colleague Dörpfeld. All now know that the latter succeeded splendidly in the undertaking by two campaigns in 1893 and 1894. In these he accomplished the tremendous work of discovering and clearing the magnificent fortification wall which is the outstanding monument not only of Troy VI but of Troy as a whole. He also found some houses of the period which were misleadingly given the much abused name of 'megaron'. The plans in photos 446 and 447 show how much the American excavations have added to our knowledge of Troy VI. They have added to the great fortification wall and made the view of the east wall much more imposing. They have given us other houses of the period, and by a methodical and meticulous study of the stratigraphy have given us a valuable history of the development of this important stage in the evolution of Troy and its relations with the external world, especially that of the west and Mycenae. What we would like is a detailed study devoted to the houses similar in treatment to the excellent account of the walls. It may be objected that the information is already all in the book. This is true, but the reader has to quarry it out, and such quarrying is always done much better by the excavators themselves.

It has, of course, long been recognised that Troy VI was the Trojan settlement, both of the Middle and of the Late Bronze Age. The continuity of the evolution of culture from the Middle to the Late Bronze Age which is obvious at Troy shows that there was no ethnological or cultural break between the Middle and the Late Bronze Age. This is paralleled by the development on the Mainland, where Late Helladic evolves gradually from Middle Helladic under the impact of outside influences, such as that of the Minoan culture from Crete. In view therefore of the space of time it covered and the striking character of its ruins, the Sixth Settlement of Troy unquestionably deserves the honour of occupying the whole volume in this series which Professor Blegen and his colleagues have devoted to it. It was a royal stronghold, although the builders of the ninth settlement, by removing the centre of the mound, must have destroyed all that remained of a palace and the more important buildings. Troy VI was not, however, Mycenaean as it is sometimes erroneously called. The Mycenaean pottery is imported not indigenous. Its walls and its houses both have a definite and a different style and plan. Even if the presence of Grey Minyan ware, both in Troy VI and in the Middle Helladic period on the Mainland, is held to indicate a racial similarity, the material remains in the two regions demonstrate that the two strains though akin had diverged, perhaps owing to the differences of their environments, the one Anatolian, the other Aegean.

Troy VI marks a new era in the history of Hissarlik. New features manifest themselves everywhere, in architecture, in ceramics, and in general culture. As already remarked, it was a royal castle in which the walled area, as for instance at Mycenae, was reserved for the prince and his immediate entourage of officials and guards, while the ordinary population would have lived outside in settlements in convenient positions in the neighbourhood. The wonderful walls with their towers, gates, and massive and regular masonry differ in style from sector to sector, and were probably not all built at once, but gradually in three separate sections, west, east, and south in that order. The large houses, with their massive stone foundations, their size, the use of interior columns, their simple, straightforward plans, their 'detachment' are in strong contrast to the undistinguished, somewhat crowded buildings of the three preceding settlements.

The discovery of a cremation cemetery belonging to the late period of Troy VI suggests a change in burial customs, but the study of the strata within the citadel nowhere showed the presence of any intrusive elements that could account for the introduction of new burial customs.

Materials now in use which are conspicuous are paste and ivory. The former was known from one solitary bead, possibly belonging to Troy IV, and the latter from only one certain



piece in Troy II. Paste beads were fairly common in the late period of Troy VI, and ten or more pieces of ivory also belong to the sixth settlement.

In pottery the old forms and fabrics are superseded by new wares with a new series of shapes. Grey Minyan ware has no local antecedents, for the excavators now admit that they were in error when they suggested a transitional phase between the fifth and sixth settlements. This ware is at first the same kind of pottery which equally suddenly characterises the opening of the Middle Helladic period on the Greek Mainland.

The animal bones show for the first time the presence of the horse, which appears in the first stratum of Troy VI and thereafter consistently in all the succeeding deposits. We are thus led to believe that the new people of Troy VI, who built the great walls, brought with them the domesticated horse.

The excavators divide the total deposit of Troy VI into three subperiods, Early, Middle, and Late. In the first there are some Middle Helladic pieces. In the second there is a steadily growing influx of Mycenaean wares. In the third actual Mycenaean vases were imported in considerable numbers, and the Trojan potters made many attempts to copy them. Some of the Mycenaean vases were probably imported for their contents, but others represent shapes which could have been brought in only for their own use. The finding in House VIF of at least five large stirrup jars can be paralleled by the frequency with which such jars are found in houses on the Mainland, as at Thebes, Tiryns, Zygouries, Mycenae (House of Columns, House of Oil Merchant, House of Wine Merchant).

Troy VI looked westward. This is shown by the likeness of the Minyan Ware to that of the Mainland, and by the frequent imports of Mycenaean pottery and other objects. The Mycenaean Ware found begins with L.H. I and II in the Middle Subperiod. In the Late Subperiod L.H. III ware is common and, though much of it is clearly of the A style, in the last phase, VIIh, of Troy VI L.H. IIIB pottery was clearly established in use as is demonstrated by the pieces illustrated in the plates. With the Hittite world contacts seem to have been rare, but with Cyprus there were undoubtedly connections proved by the presence of Cypriote pottery in Late Troy VI. On the other hand, Trojan ware has been found in Cyprus and in Palestine.

The Sixth Settlement perished in a violent earthquake—was this Laomedon's Troy which was in legend connected with Poseidon the earthshaker?—of which the excavators give plentiful proof. It would have taken place after L.H. IIIB pottery was in use. The development from L.H. IIIA to L.H. IIIB the excavators place about 1300, which in our view is too late and is based on Furumark's ideas which they (p. 20) do not accept. We would place the transition from L.H. IIIA to B after the close of the Amarna Age about 1350–1340 B.C. and so give L.H. IIIB, which to judge by the great amount of pottery belonging to this style must have enjoyed a long life, a span extending at least to the end of the reign of Merneptah. The beginning of Troy VI is placed by the authors about 1900 B.C. or even later, but there is no certainty as yet for such absolute dates. In any case, Troy VI seems to open about the same time as the early Hittite kingdom, and the sudden appearance of Grey Minyan suggests that its beginning was also approximately contemporary with the commencement of the Middle Helladic period. We have reason to believe that the Middle Helladic people were Greeks. The Hittites were Indo-Europeans. If the new Trojans of Troy VI were akin to the Middle Helladic folk they presumably were also Indo-Europeans. We would thus have what was practically a simultaneous movement at the beginning of the Middle Bronze Age of Indo-European peoples (with horses?) into these areas stretching from Central Anatolia in the East past Troy in the centre to Pylos in the West. Troy, especially Troy VI, is a key site in this great Indo-European invasion, as we assume, of the Near East, and for this reason alone we welcome this impartial and admirable presentation of their results by Professor Blegen and his colleagues. *O si sic omnes!* We look forward eagerly to their fourth volume describing in the same masterly fashion the results of the excavation of Troy VII, the first stage of which was Priam's city.

Dr. Lawrence Angel's book is the first of a series of monographs planned to accompany the main account of the excavations at Troy, and other volumes will in due course deal with animal bones, coins, and various analyses. Dr. L. Angel handles the rather scanty human material with his usual scientific thoroughness. He deals with only forty-five skeletons or portions of skeletons, for some are very fragmentary, especially those from the urn burials in the cremation cemetery of Troy VI. With the new Trojan material Dr. Angel has included older material, previously described by Virchow and Houzé, from North-western Anatolia and sites like Therme in the neighbouring Aegean area. In spite of the scantiness and

character of the material, which cannot be analysed by statistical methods, some interesting points emerge. The infant mortality was apparently high and indicates 'the high fertility and youth of the population and the rigours of life and selective processes in the first walled cities'. He concludes that 'race mixture was dominant at Troy from its foundation onward' and that 'racial continuity is illustrated by reappearances of various traits as well as by persistence of the Mediterranean type. The similarities of the Trojan skulls are with the east, south-east, Aegean, north, possibly in that order of importance.' Dr. Angel most wisely disclaims any certainty for his results, which are obtained rather by implication, and says that 'the essence of the racial situation in a growing civilization is Heracleitan flux'. He has set forth his evidence clearly and scientifically, and the facts so far available are presented for the inspection and study of all students and scholars. Dr. Angel is to be congratulated on an excellent report which is of permanent interest to all concerned with the prehistory of the Near East.

A. J. B. WACE.

**The Coming of Iron to Greece.** By T. BURTON BROWN. Pp. 329. Privately printed by J. R. Hawkins, 119 Woolwich Road, London. Price 30s.

There are certain devices and motifs that are distinctive enough to present a challenge: why do they appear at certain times in particular places; why do they not infrequently disappear, to emerge again centuries later; how did they arrive? The same questions are asked by new shapes in metalwork, by techniques and inventions and ethnological practices. Some of these phenomena have long been themes for controversy, others present themselves as fresh discoveries multiply. Though the last word may never be said, much information can be gathered by carefully examining what are obvious clues.

Mr. Burton Brown has collected not only the more familiar of such clues but also a large number of others, and his collection is accompanied by discussions and an interpretation. Thus the book has a scope much wider than the title suggests and an appeal from at least three aspects. Because the 'collection', submitted in a series of catalogues interspersed with the text, has an independent value, it is here reviewed first. Readers may not agree about the relevance of all the types and subjects comprised; they may wish to subtract from or add to the examples quoted, and are, indeed, invited to do so; but they will find themselves constantly using those lists, of which the full significance only gradually becomes apparent. They will, however, regret that Saul Weinberg's important article in *AJA* LI has been overlooked when they come to the 'horned' or 'Macedonian' handles on p. 187.

Naturally, the discussions form the main body of the book. Some deal with the subjects of the catalogues, some with other aspects of archaeology or with historical and ethnological topics bearing on the interpretation advocated. Here, too, the range is so great that omissions are inevitable, and each specialist who reads will, no doubt, produce criticisms relating to his own field. Anatolian experts, for instance, will think that more should have been said about textual evidence for iron and for cremation among the Hittites: the actual cremation burials at Bogazköy were, of course, discovered too late for insertion.

A basic principle thus arises: how far do omissions, which involve the possibility though not the certainty of misinterpreting the evidence, detract from the book's value? The answer, for one reviewer at least, is that gains definitely outweigh losses. There are some very useful sections that might some day be expanded into separate articles (those, for instance, on tripods and fibulae, provided that the proto-fibulae from Alaja are also considered). There are, moreover, suggestions, arising from the breadth of the field covered, which are new and stimulating; references which are relevant but unexpected; all these must not be missed. The presentation is sometimes awkward, unorthodox; reading is not easy; but it is certainly rewarding.

The conclusions must now be summarised. Mr. Burton Brown believes that the introduction of motifs, inventions, and practices is due to the arrival of migrants; that the recrudescence of these phenomena after periods of eclipse can be attributed to further arrivals of kindred peoples; and that thus we can account for certain major changes in the Aegean civilisation and elsewhere. Greek lands would be subject to such changes, notably in the sixteenth and thirteenth centuries and at the beginning of the geometric period. To what extent can this theory be reconciled with any of the familiar views on the origins of Minoans, Mycenaeans, and Greeks; views based, to a certain extent, on philology and tradition, as well as on actual remains? One needs to be told. References in the text to relevant literature could have clarified the issue.



In estimating the claims of migration as against evolution, Mr. Burton Brown may have more support than he realises: pp. 19-20 overstate the strength of the opposition. On the other hand, his belief that trade was a comparatively unimportant factor can scarcely be acceptable: ample literary evidence about its wide ramifications exists, such as the records of the Assyrian merchants at Kültepe. And surely no influx of peoples, even on a comparatively small scale, could have come to Greece after 800 B.C. (as also postulated) without notice by Greek historians.

All movements of peoples reached Greece, it is suggested, from the east, having passed at some time through the Caucasus. One can but hope that, some day, more stratified material will be forthcoming from that region, to which Mr. Burton Brown's own excavations in Azerbaijan have taken us one step nearer. In the meantime, his book, in spite of the points challenged above and the reserve accorded to several conclusions, deserves our attention.

W. LAMB.

**Neue Beiträge zur klassischen Altertumswissenschaft. Festschrift zum 60. Geburtstag von Bernhard Schweitzer.** Ed. R. LULLIES. Pp. 419, with 91 plates. Stuttgart: Kohlhammer, 1954. DM. 46.

Schweitzer is a scholar who combines range with depth of penetration, and these qualities of his scholarship are reflected in his *Festschrift*. The distinguished contributors have exerted themselves to give of their best; printer and block-maker have done them proud, and the book may rank as a *Festschrift* of outstanding merit. Two thirds of the total of fifty-eight articles are appropriately on Greek subjects, and show a marked inclination towards art and archaeology; in fact, only Berve's essay on the scope of Pyrrhus' kingship in Sicily and Paul Maas' restorations in Bacchylides fr. 20 A belong to other fields. Roman studies, not under consideration in this review, have a smaller but spectacular representation.

The book opens with two capital articles on Mycenaean subjects. Wace most satisfactorily locates all the monuments seen by Pausanias, with the sole exception of the 'Grave of Atreus', which he is inclined to identify with the tumulus surmounting the beehive of Clytemnestra; if Pausanias' Grave of Atreus need be sought outside the Shaft Grave Circle, this seems the best bet. Marinatos makes some good points about the chalice with the falcons (ex-doves) from the Fourth Shaft Grave. He justly points out that Nestor's *depas* in Iliad XI is not a cup (presumably the point of the four handles is that it would take two ordinary people to lift this mixing-vessel when full); but when he tries to explain it by comparison with double-bottomed Cretan utensils of specialised uses he carries less conviction. Figured handles with two *mbēves* underneath could spell column crater, and if R. S. Young is right in regarding the column crater as a metal form of Geometric times and not an Early Corinthian potter's invention (*Hesperia Suppl.* II 173 f.) I should suggest this as a possible solution.

Several articles recall Schweitzer's pioneer work in the Geometric. Weickert sketches an evolution of Proto-geometric ornamentation which leads to the abstract picture. Kraiker, on the other hand, sees in the Geometric picture something evolved as distinct from the ornamentation, governed by its own rules, with narrative replacing rhythm as the prime factor; in insisting that the Dipylon vases depict typical actions and not particular incidents, he ranges himself with British rather than German scholars. In a masterly article Kunze inaugurates the serious study of the great figured vases; by reassembling scattered portions of two Dipylon craters he discovers one of the front-rank painters and sets him in his relationship to other artists of the classic Geometric. According to my notes, a fragment with legs in Athens should give the bottom of the combat-zone of pl. 8 and a subsidiary band of cross-hatched triangles below, but my attribution may be at fault.

The bulk of the articles deal with archaic topics and sculpture of the great period. Buschor briefly appraises the two great Samian marble sculptors—the Master of the Hera and Genetrix. He brings to light two further dedications by Cheramyes, who now appears as having set up three handsome offerings over a span of twenty years or so; who was this man with the Carian name? Von Gerkan vindicates the correctness of Koldewey's observations at Neandria against Doerpfeld's and Schefold's surmises that the temple had a peristyle, and thus re-establishes Koldewey's three-piece Aeolic capitals; this seems just, and I would add as corroboratory evidence the recent fitting of a roll member to a Klopevi volute-piece (Kondis, *Ann.* XXIV-VI 28), as well as the discovery of a similar open walk along the edge of the earlier platform of the seventh-century temple at Smyrna. Krauss demonstrates that the cornice of the Lucanian Heraeum had no geison; he then

argues, less cogently, that the roof-tiling must have ended in antefixes (of which, however, no trace remains) behind the trough of the sima, thus presenting a double crest for which he seeks comparisons in later classical roofs like the circular Lysicrates monument. Hampe rejects the apple that Dawkins put in Paris' hand on the Orthia Judgment comb, seeing in this intrusion an element which had no place in the archaic iconography of this scene. S. Morenz regards the *Batrachomyomachia* as derived from the Egyptian war of the mice and cats and probably of the sixth century B.C. In connection with the Theban tale Lullies re-illustrates the Arndt seal with the human-bodied sphinx reading a scroll; her dugls look Minoan, her head archaic, the bottom is a monkey's.

Beazley makes amends to the potter Nicosthenes by publishing a fragmentary cup of his painted by Lydos. Züchner attempts to cap the Amasis amphora in Würzburg with a lid in the same collection. Bielefeld publishes from photographs an Ekeian amphora with a striking painting of a four out of hand, and Hermine Speier restores the design of the Euphronian Iliou Persia cylix in Berlin with the help of fragments in the Vatican. An engaging note by Lippold on three vase inscriptions appeared after his death.

Karouzos contrasts the motion and rhythm of the Omphalos and Kassel Apollos (identified respectively with the Alexikakos of Calamis and Phidias' Parnopios); he finds in the narrowed stance and the relief character of the Kassel statue the developed Phidian conception of the god. Krischen reviews fifth-century arming scenes in which a female figure hands the warrior his arms, and suggests that the Lemnia of Phidias was not herself depicted as under arms but as proffering arms to the cleruchs to sanctify their armed departure; though without parallel, such an action on Athena's part harmonises with the gesture of the statue. Brommer shows that the fragment Smith no. 127 belongs to the lower part of the Iris of the Parthenon west pediment; this gives a fine swirl to the drapery at the back, and at the same time elucidates the inclination of the statue and the securing of the flying figure in the pediment. Wegner seeks to attribute two fragments to the charioteer of Athena in the same gable. The major one, comprising the lower legs of a figure in chiton and himation, seems to fit the position admirably, but the calm vertical fall of the chiton around the feet disagrees not only with the Anonymus Nointel but with the spirited movement of the horses, which must surely have been transmitted to the charioteer. Ashmole exposes some nineteenth-century forgeries of heads from the frieze slabs on the Acropolis.

Harder handles with some adroitness two much-discussed artists' signatures—that of Paeonius' Victory, and the Columna Naniana on which he prefers to take γρόφον as the sculptor's name. The explanation of *ἑνὸς* at Olympia as a kind of 'Antimperfektum' does not remove the difficulty, since that imperfect is one of duration and the main action of the decree or dedication cannot fall outside its limits; in assuming without discussion that the competition was not an 'echte Agonistik' Harder does less than justice to Paeonius' words. Among the old favourites, the problem of authorship of the 'Venus Genetrix type' is reconsidered by W. Fuchs, who stresses the mannerist treatment and plumps for Callimachus. Vagn Poulsen sees likely copies of the portraits of Cimon and Phidias in two heads in Copenhagen.

Lysippian studies are to the fore. Kleiner gives the findings of a seminar from which the master emerges enriched; Koch, on the other hand, robs Lysippus to pay Leochares. Consonant with the recent discovery that Lysippus was already active in Pelopidas' lifetime, Schuchhardt looks for works of the long-lived sculptor's youth and recognises one in the Ny Carlsberg-Dresden Herakles. Might the same not be done for Phidias, of whom it was agreed that he was a bald old man in 440 B.C.? Luschey discusses the re-emergence of symbolic creatures in Attic sepulchral art from the end of the fifth century, and Miss Richter makes a point in support of Friis Johansen's view of the relationship of living and dead on the figured stela. Two marble heads of children in Göttingen are published by Horn and W. H. Gross. Curtius in a last flash of genius traces back the scheme of the high priest's servant in a late fifteenth-century Flemish Betrayal to the Attalid dying Persian in the Vatican.

To conclude with the miscellaneous matter, Nilsson suspects that the little archaic terracotta altars of Corinth are house altars, like those of Olynthus and Magna Graecia, and would trace the custom back to the shadowy Mycenaean offering benches; in the present state of our knowledge this seems a longish shot. Webster scores a point or two on representations from Attic comedy. Herbig throws a searching light on the demi-monde of the Etruscan mirror. And Vogt re-establishes the church of the Holy Apostles at Constantinople as the foundation of Constantine the Great and not of his son Constantius.

J. M. COOK.



**Protoprogeometric Pottery.** By V. R. D'A. DESBOROUGH. Pp. xvi + 330, with 38 plates and 1 map. Oxford: Clarendon Press, 1952. 105s.

This is an important book. In effect it is a museum catalogue of nearly all the Protoprogeometric vases in Greece, and they are a vast company. The worst of it is, that the people who need its guidance most, poor students and travelling scholars, cannot afford it. On the other hand, the vases it illustrates and describes so faithfully are too abstract in their decoration to appeal to the average run of more opulent dilettanti. If only the author had allowed his generally clear illustrations to speak for him sometimes, how much bulk might have been saved. We can, however, be grateful to him for the clarity and ease of style with which he sets before us a prodigious amount of learning.

The first half of the book concerns Attica, and henceforth no one will be inclined to dispute that Greek Geometric style originated in Attica. If parts of the argument in this section are not always immediately easy to follow, the difficulty arises from the nomenclature and arrangement of *Kerameikos* I and IV, which our author has to use: he might have made it easier for us by adding a few more references. I am particularly grateful for the illustration of a new pyxis in London (pl. 13). Clearly this must be an ancestor of the Protocorinthian 'Tall Pyxis' shape, and no one will again be disposed to look for the prototype of this shape in Crete. We do not see the shape again for 150 years, which must mean that storage vases were not fashionable in Attic graves. We must suppose that a similar eclipse overtook lekythoi also, for that shape, too, reappears much later. Its place in Early Geometric graves was taken by the aryballos.

The second half of the book deals with less well-known material, scattered all over Greece. Its remoteness explains, although it does not commend, disturbing shadows and regrettable backgrounds in some of the photographs (see pls. 16-20).

D. is no controversialist. This part of the book might have been so dull, had all the combatants been mentioned by name, and if all their theories had been brought before us to be destroyed. Sometimes D. just sets out the facts remorselessly and leaves the theories to fall painlessly apart, as in the matter of skyphoi (I prefer the term, 'cups'), with overlapping semicircles (180 ft.). Sometimes he alludes obliquely to a past debate, e.g., as regards the objects from the multiple Tombs of Marmariani (147). He is alluding to the spectacle fibulae which have been used to date up the European Bronze Age. Incidentally, Mr. Andronikos tells me that he has found spectacle fibulae in tombs with fourth-century Greek vases. Sometimes D. can be observed delicately shifting his ground. Speaking of Kephallenia in a former paper (*BSA* XLIII, 268), he said that he saw no reason for 'calling these Lakethra vases anything but provincial contemporaries and descendants of latest Mycenaean'. Now he admits (272) that 'the period of time covered' (i.e. its lower limit) 'cannot accurately be determined'. In other words, the tombs do not confer a Mycenaean date on spear-heads of Northern type found there. It is characteristic of D. that he notes the Northern connection (297), but ignores the question of date.

Two criticisms: (1) D. should not allow his love of paradox to decoy him into subtleties of terminology which defy archaeological practice and logic. Decoration is not a way of painting (123). The main decoration of a decorated vase must be a design of some sort, it cannot be any way of painting, dark on light, b.f., r.f., or the like. However humble this decoration may be, whether it is a couple of inscribed semicircles on the shoulder or a great scene splashed all over the vase, it remains the main decoration of its vase. It is a fallacy to call it 'subsidiary decoration'.

(2) D. exaggerates the difficulty of travel, and this has led him to deny to East Crete a Protoprogeometric style of its own. If their style is not derived from Knossos, and here D.'s judgement must be accepted, there are enough close resemblances to Protoprogeometric painting in the *Kerameikos*, and in Ithaca, to make us call the vases in the Chamber tombs at Vrokastro 'Protoprogeometric' (see 262), as well as the metal objects found with them (308 ft.). I am particularly struck with the resemblance of the decoration on the shoulder of the belly amphora in *Kerameikos* I, pl. 58, no. 563, Hall, *Vrokastro*, pl. xxvii, 4 and *BSA* XLVIII, 269, p. 142, from Aetos. Even the small neck of the oinochoe is characteristic of Ithacan Protoprogeometric, pouring vases. Cf. also the ends of the pattern of Hall, *loc. cit.* 2 and of *BSA* XXXIII, 43, fig. 15. Juxtaposition of cross-hatched and diced diamonds can also be seen on a kantharos from Polis, Ithaca, on D. pl. 6 from the *Kerameikos* and on the pyxis from Vrokastro under discussion. D. convinces me of contact between Ithaca and Laconia (288). I mentioned a Protoprogeometric import from Ithaca in Olympia, *BSA* XLIV, 309, fig. 1, No. 2, and now we have an Ithacan Protopro-

metric sherd in the Taranto Museum. Travel was not, then, too difficult in the Protoprogeometric era.

After these few grumbles, it only remains to congratulate D. on writing the standard work on Protoprogeometric pottery in Greece.

SYLVIA BENTON.

**Kerameikos. Ergebnisse der Ausgrabungen. Vol. V, 1. Die Nekropole des 10. bis 8. Jahrhunderts. Text and Plates.** By K. KÜBLER. Pp. viii + 310, with 4 plans. 167 plates. Berlin: De Gruyter, 1954. DM. 178.

Vol. V, 1 covers the Geometric graves on the banks of the Eridanos. There were a hundred of them, and all save eight were uncovered by the Germans in the years 1932-40. Close attention in excavating and diligent study of the finds have brought their reward, and we can at last consider ourselves adequately informed about the Dipylon graves. The Submycenaean and Protoprogeometric had been dealt with in Vols. I and IV, and the publication of the cemetery is now completed down to the late eighth century. The Attic Orientalising, the last of the great grave series, will (we hope) follow shortly. Dr. Kübler regrets that owing to the difficult conditions his description and illustrative cover (especially of the metal objects) is not always complete; but though a few drawings would have come in handy, the deficiencies are not serious. The production is excellent and the photographs splendid.

In the first chapter Kübler presents the factual evidence and discusses the spread of the cemetery, the orientation of the graves, funeral customs (with the alternation of cremation and interment) and cult of the dead. He attaches himself to the view that Attica was more or less overrun at the end of the Bronze Age; and he sees in the burial habits of the succeeding centuries a growing revival of the older beliefs and ritual, Dionysiac and still more Eleusinian. It is interesting that finger rings and hand-made crock pots are generally found in women's graves, while pyxides and gold head-bands seem to belong to male burials.

The weight of Kübler's research is directed upon the vases. The different forms are examined, and their development is explained. For the transition from Protoprogeometric and the course of Early Geometric in the late tenth and ninth centuries this work will not need serious revision; the contemporary Attic material outside the *Kerameikos* is not abundant, and Kübler has been able to take it fully into account and incorporate it in his scheme. His absolute dating at this stage depends on Tel Abu Hawam, which he uses to maintain the relatively high time scale (see, however, Desborough, *Protoprogeometric Pottery* 294). The art-historical analysis demands patience and a clear head. It seems to incur two main dangers. First, the things that are significant tend to get swamped. The second danger comes from pushing to extremes the distinctions that have been formulated. Grave 12 will serve as an example. Four of the five vases found in the grave (including a cup no. 892), together with the vases from the pyre above it, are dated around the beginning of the eighth century (except for one conical-footed bowl, apparently much earlier); but the cup no. 893 from the grave, though at first sight almost identical with 892, is dated in the second quarter of the century on art-historical grounds; and thus the grave is dated a generation or so later than nine tenths of the vases found in or over it. But among the obvious doublets (i.e. pairs of vases made in one workshop as part of one order) we find much more marked differences than between 892 and 893 (e.g. the sister bowls (pl. 120) from grave 24, or the lids (pl. 123) of the pair from grave 10). It is difficult to resist the conclusion that a critical instrument has been forged which—when applied to the minutiae of lesser vase forms—is apt to register with a greater precision than the Geometric craftsman's hand was capable of. In nomenclature Kübler is unexpectedly fluid; after the phases of the Early Geometric we hear only of High and Late Geometric, and the distinction between the two is not pressed.

The wider connections of the later Geometric pottery of the *Kerameikos* have been reserved for a special supplement; but the important question of Late Geometric chronology is dealt with at length. Kübler lays full emphasis on the kotle as the one direct link with the western foundations, and in arriving at a date in the early thirties of the eighth century for the two 'Opferinnen' he justifies a somewhat earlier dating than most scholars have affected in recent years. But in cramming into the inside of a decade the whole development from the 'Rinnen' to the incipient Early Orientalising of no. 1370 (pl. 40, cf. the Oxford amphora 1935. 19, *BSA* XLII 150) he is less convincing. On p. 173 he appears to bring the Dipylon craters also down into the same brief span, so that nearly all the rich development of Attic Geometric representational art is concen-



trated in his scheme between 745 and 735 B.C. The graves of the last generation of the Geometric in the German Kerameikos have proved a disappointment. They have added little to our large stock of handsome figured vases of this time, and what they have added is mainly second-rate. The greater part of it seems to adhere to the ever-expanding circle of Athens N.M. 897 (cf. *BSA* XLII 144 ff.), and might almost justify the use of the term mass-production. The graveyards on the banks of the Eridanos seem in fact to have sunk in the social scale; Dipylon craters did not come this way, and relatively few of the handsome figured amphorae and hydriai of the close of the Geometric have been found in urban cemeteries. This poverty has, I think, led Kübler into underestimating the length of the road which the Late Geometric vase painters travelled. It is only through the study of the interplay and development of traditions and painters in the light of the many dozens of figured vases in other museums and collections that a precise arrangement will become possible. But though Kübler's congestion may need to be eased—the early Orientalising downwards into the last quarter, and graves like 25 and 26 upwards in the first half of the century—his fixed point of the thirties for the 'Opferinnen' seems likely to provide a secure terminal for the absolute chronology of the last stage of the Attic Geometric style.

It remains to congratulate Dr. Kübler on this massive achievement and to hope that his publication of the remaining grave-sequences will not be beset by so many difficulties as the present volume has been.

J. M. COOK.

**Geschichte der griechischen Kunst, I. Die geometrische und die früharchaische Form.** By F. MATZ. Pp. 328; 297 plates. Frankfurt, 1950.

This is the first volume of a full-scale history of Greek art, written from an individual point of view. The first instalment carries the story from 1100 to 570 B.C., and is accompanied by a volume of plates and illustrations, which contains good pictures of a considerable proportion of the surviving works of sculpture, painting, architecture, and of the minor arts. All these works, which amount to something like 450 in number, are described, and the descriptions include adequate bibliography. We have therefore the illustrated commentary of a very good scholar on a period of extreme importance. Much of this commentary is both sympathetic and penetrating, particularly the discussion of geometric painting and the characterisation of Early Attic art as showing 'an inflammable imagination'. English readers may well find difficulty with the general ideas—not only the axiom that structural constants should be referred to particular peoples or races (I am doubtful whether it is useful to discover Indogermanic elements in archaic Greek art) nor the rather cryptic equation between early archaic art and contemporary political, religious, and literary phenomena, but also the general concept of *Strukturforschung*, which is discussed in the introduction and to which the author returns in his individual interpretations. The reviewer would have welcomed instead more detailed discussion of individual monuments, e.g. mythological scenes in geometric (why are the Attic Aktorion omitted? what is the significance of the so-called Dipylon shield? why should the figure on the Tiryns shield be Herakles rather than Achilles? who is the Chiot lion-fighter?), but the author undoubtedly regards *Strukturforschung* as his chief object, and in spite of its difficulties his book is a major contribution to the study of early archaic art.

T. B. L. WEBSTER.

**Heraion alla Foce del Sele I, Il Santuario—Il Tempio della Dea—Rilievi figurati vari; L'Architettura a cura di F. Krauss. II, Il primo Thesaurus.** By P. ZANCANI-MONTUORO and U. ZANOTTI-BIANCO. 2 vols. of text, pp. 214 and pp. 390; 2 portfolios of plates I–LXIX and I–CIV. Rome, Libreria dello Stato, 1951 and 1953. Price about £35.

The review of the first volume has been delayed until the appearance of the second, since both volumes are closely linked.

The superbly produced work is the final publication of the excavations undertaken from 1934 to 1940, and in the spring of 1949 by the authors at the shrine of Hera (there can be no doubt of the goddess honoured, as the terracottas in particular show) located at the mouth of the Sele (Silaris) River near Paestum in S. Italy. The interim reports have appeared in *NS*, *AA*, and *JHS*. The present work contains a full account of the structures and figured reliefs found on the site, and a great deal of very full general discussion of the sanctuary of Hera from historical, artistic, and religious standpoints, against the background of Italiote Greek culture. There yet remain to be published the pottery, coins, objects of bone, metal, and terra-

cotta found on the site. For the most part their contribution to the dating of buildings and sculpture is small and doubtful (see below). The publication of the most recent finds made from 1950 on is promised in *Atti e Memoria della Magna Grecia*.

The history of the site, as it emerges from the excavation, remains obscure in many details. The excavators are inclined to regard the site as part of the earliest settlement of the Greeks who afterwards established Poseidonia: one of those headland or island preliminary settlements common elsewhere (I 22–4). The sanctuary here established by the incoming Greeks not far from the later port of Poseidonia retained subsequently the regard and reverence of the inhabitants of Poseidonia—Paestum, especially as the centre of a fertility cult of Hera (excellently discussed (I 9–19) in its manifestations here and elsewhere, with an interesting account of what seems to be a modern survival of pagan cult practice). At a later date, indeed, the fortunes of the sanctuary and the city of Poseidonia were affected by the fall of Sybaris, but for the earliest foundation the excavators prefer (I 10, 140) the tradition which connected Poseidonia, or rather the temple, with the Argonauts (Strabo V 252). This is taken to indicate a Thessalian–Bocotian strain, as well as the Troezenian element represented by refugees expelled from Sybaris (Aristotle, *Pol.* V iii 12, 1303a29) who may have settled at Poseidonia, while there was a later accession of Sybarites expelled from their homes by the fall of their own city. Whatever the antiquity of the site (see Dunbabin, *The Western Greeks* 25–6, and below) Strabo's Greek will hardly bear this interpretation, and Dunbabin's suggestion is better (*op. cit.* 26), that the high antiquity of the site on the sea, from which the Sybarites (?) moved inland, brought about the traditional association with the Argonauts. This 'high antiquity' of the settlement at the Silaris mouth, tentatively accepted by Dunbabin, is one of the problems which await solution or clarification by the publication of the small finds; and it is a great pity that room was not made in these two volumes for the publication of at least a selection of the pottery finds mentioned briefly in I 25 as including Protocorinthian as well as Corinthian (leaving aside the sub-Mycenaean *askos*). It would not be the first time that imitations were confused with originals, and dated too early, just as some of the ivories from the recent excavations at Paestum (*AA* 57 (1953), 214 and pl. 61, 4), which seem to be native (?Etruscan), are taken (*ibid.*) to be imported Ionian Greek. Some of the terracottas from Foce del Sele have parallels at Perachora (*Perachora* I 191), which are dated to the early sixth century.

With the arrival of the refugees from Sybaris, at the destruction of that city, the excavators connect the erection of the temple of Hera treated in Vol. I (replacing an earlier structure of which only traces remain). The disaster of the attack and occupation by the Lucanians in the early fourth century, their expulsion by Alexander of Molossia (between 336 and 330 B.C.), and their return to dominate the Greek city (though the cult seems to have retained its importance) are seen to have left their marks on the sanctuary; earlier structures provided building material for the later, and thus some of earlier sculptural decoration was preserved, and its discovery makes a precious addition to the scant remains of early Italiote art.

The excavations have revealed in the sanctuary four buildings dating from before the Lucanian attack: A Doric peripteral temple, the altar before the temple (at a greater distance than usual, and not co-axial with the temple), a smaller Doric building called by the excavators 'Treasury' I (see discussion of the term in II 9–10), though the new discoveries at Paestum itself justify doubts as to the use of the term, and an archaic 'stoa' used, the excavators suggest, as a place of purification for those presenting themselves at the sanctuary. All these are in what is called 'Zone A', as is also a complex of structures seemingly serving the same purpose as the archaic 'stoa', and pretty clearly dated after the period of destruction, and forming part of the restoration. Some distance away, in 'Zone B', are remains of walls and of a tower, also of later date.

Of the earlier buildings relatively little remains *in situ*, but enough of ground-plan and scattered architectural fragments remain in the case of the Temple and 'Treasury' I to permit convincing reconstructions by Friedrich Krauss and the excavators. Of special importance are the sculptured metopes; some were lying fallen or dismounted on the ground, others were built into the later structures, from which their recovery has been a miracle of patience and skill, of which the excavators are justly proud. A very useful list (I 63–6) analyses the sculptural finds. The great bulk of the metopes and fragments are associated with 'Treasury' I and are fully treated in Volume II (see below). To the archaic temple are allotted metopes nos. 45, 46, 64, 70, 89, and a number of fragments which are important only as an indication of a diversity of subject (no. 10 'Frammento di busto con chitone e balteo'; no. 12 'Metope incompleta con arciero inginocchiato'). No. 9, a metope with



a representation of Herakles, trimmed for later use as a stele base, may be part of the decoration of the temple as restored after the disastrous fire and damage of the Lucanian onslaught. The fighting hoplite metope (no. 47), of different dimensions from the foregoing, and certain other fragments, may represent a Greek-Amazon battle, and are tentatively allotted to another small building ('Treasury' III) of c. 480 B.C., though all other traces of it have disappeared. One is more inclined to accept these evidences of 'Treasury' III than the far more dubious evidence for a 'Treasury' II: the 'misere tracce' (as the authors themselves say, I 173) represented by no. 38, and the portion of a head (no. 24) render dubious the authors' stylistic judgements here, and also the suggestion that these fragments and a Doric capital (fig. 49) belong to another 'Treasury' (II) to be dated between 540 and 530 B.C., perhaps to be ascribed to Velia. This is imaginative reconstruction with a vengeance, and indeed throughout the publication the authors' infectious enthusiasm for their discoveries manifests itself in an almost uninhibited exercise of the imagination. But let it be said in all fairness that conjecture is never represented as anything else but conjecture.

For reasons of space the archaic 'stoa', the 'Treasures' II and III, and the later structures may be passed over. The temple and 'Treasury' I, and especially their sculptural decoration, represent the objects of greatest interest.

The temple of Hera treated in Vol. I was a Doric peripteral structure (8 x 17 columns) 38.9 metres long and 18.6 metres wide, with pronaos (distyle in antis), naos and adyton; the peristyle is two intercolumniations deep at back and sides, and three at front. Of the ninety-two metopes of the reconstruction only five survive more or less complete. There is no space to mention the problems of the reconstruction from scattered fragments where relatively little is preserved *in situ*: some of the problems are of great interest, e.g. the presence or absence of angle contraction, the position of a staircase or staircases to the roof. Suffice it to say that the whole reconstruction is well discussed by F. Krauss and the excavators, and illustrated by admirable drawings. It is worth quoting the general characterisation of the temple (35): '... il nostro edificio prende posto tra i tempi del periodo di transizione, con alcuni caratteri ancora arcaici, come il numero inconsueto delle scanalature delle colonne, come la persistenza della rastremazione dei triglifi e dell' obliquità delle metope, come il profilo delle sue cornici, e con altri più maturi come il perfetto legame della cella con la peristasi e come la contrazione degli interassi angolari. Infine rivela influssi marcatamente ionici nella pianta, nella decorazione dell'elevato e nella concezione del pronaos.'

There is no substantial evidence for dating: the pottery finds help little; the style of the metopes (see below) seems, with certain characteristics of the structure, to determine the date arrived at, viz. 510-500 B.C., the impulse being given by the fall of Sybaris and the flight of refugees to Poseidonia. In any case there must be something of a gap between the conception of the temple and the execution of the metopes, though not necessarily a very long one. How far is this dating an independent conclusion from the evidence, or how far, like the excavators' dating of 'Treasury' I, based on a historical preconception? The present writer does not feel competent to speak of the purely architectural considerations, but, it may be asked, are the evidences of 'transition' effectively such, or evidences of lack of skill (cf. slotting of some metopes and pure juxtaposition of others with the triglyphs)? The metopes are a sad ruin, for little can be guessed of the subject, or subjects—for there must have been more than one; see I 123 for a description of the main surviving examples: are the figures engaged in a ritual dance or a ritual procession? There are also the small remains of what might be an Amazonomachy (no. 6 of catalogue, pl. LX). Therefore what has perished might have displayed striking differences of style, and what remains affords little opportunity for conjecture on subject or position on the building (though the authors make an attempt I 125, n. 1, and 126, n. 2). The parallel drawn between the striding figures of the metopes and the running figures of Euthymides' amphora with the Rape of Korone (Munich 2309), which gives the suggested date 510-500 B.C., does not seem altogether unsuitable for the drapery rendering, but its significance is small for purposes of dating: the reliefs may well be later, though not necessarily so; indeed, the details chosen for the comparison are approximations to renderings which go back also before 510 B.C. The heads suit the date suggested: at least some do (pls. XLI-XLIV, XLV-XLVIII), another (pls. L-LII) looks more barbarous, or earlier, and younger sister to some work of the 'Treasury' I. One wonders if they are not all imitated from vase paintings (cf. pl. XLVIII for the body beneath the drapery rendered in what might be called a painter's rather than a sculptor's style) by local artists of varying capacity. Separate

artists are detected I 131: '... potremo concludere che le prime tre metope sono certamente opere di tre diversi scultori, mentre forse allo stesso maestro che produsse il n. 3, può attribuirsi anche il n. 5' (No. 4 is too damaged). The dating is a nice point, as is the general artistic background of the reliefs, which is fully discussed I 123-40. Interesting characteristics are the heads placed against the *taenia* at the top of the metope (to avoid too high relief in the bodies?), and the absence of undercutting (contrast some metopes of Treasury I). Note also the oblique setting of the metopes (137, compare Treasury I), the taper of metopes (? down) and of triglyphs (? up), though they are too few and damaged to make the practice quite certain, and the disproportion of parts in the bodies—the two latter characteristics may be intended to secure the right optical effect from ground level.

The subject of the second volume is the small building 'Treasury' I connected by the excavators with Sybaris when first discovered. They now suggest a connection with Siris. It lies 14.3 metres from the temple (confused (?) by Dinsmoor, *The Architecture of Ancient Greece*, 85-86, with this building), parallel with it and orientated like it E.-W. Its dimensions are small (length, including porch, 16.94 metres, breadth 8.94 metres), it is of the Doric order, tetrastyle prostyle in form, divided into a deep *pronaos*, and a cella only 4.80 metres deep, in the back wall of which is a door. It was decorated with thirty-six metopes; further ornament consisted of relief-decorated anta-capitals (rosettes on one, lotus-palmette chain on the other), an elaborately decorated cornice under the architrave on the outside of side and back walls (decoration of rosettes and leaves, see 24-5) at the level of the anta-capitals, and a moulding above the rear door (26, fig. 10). No trace has survived of the horizontal cornice (*geison*), *sima*, and water-spouts. The roof is hipped (about which there can be no doubt), and the organisation of its timbers and the nature of the *geison* presents some nice problems; some assistance is given by the metope-triglyph slabs: 'Infatti non solo sul lato posteriore di alcune lastre, ma in qualche caso anche sul taglio superiore sono praticati incavi, che dovevano essere in rapporto con parti lignee sporgenti più del necessario' (40). Some of these cuttings settle the question of the hipped roof ('sul taglio superiore di alcune lastre del fregio sono stati praticati incavi per far posto alle estremità inferiori di travi inclinati', 41), but not the angle of the incline. For various reasons (see 40-1) the excavators restore a *geison* and architrave of wood (see the discussion 33); but they seem elsewhere (68), on account of the absence of an upper frame to the metopes, to suggest 'una lastra fittile nella doppia funzione di *geison-sima*... poteva completare verso l'alto il fregio dorico, nascondendone le irregolarità con la sua parte pendula'. The problems of the architectural reconstruction merit the interest and careful examination of the expert (see Dinsmoor, *loc. cit.*); suffice it to say here that in a splendid series of drawings (pls. I-XXIII) all the evidence is made available to other students.

The outstanding interest of the publication is the definitive publication of the sculptured metopes, the piecing together and preservation of which have been done with exceptional skill. These metopes are carved on blocks which for the most part incorporate the triglyph also, generally on the left of the metope; a certain number (Pholos, no. 1 (uncertain); Herakles and Hera, no. 7; Herakles and Antaios, no. 15; Head of Mourning Woman, no. 23 (uncertain); Orestes and Erinys, no. 26; Apollo and Artemis, no. 28; Tityos and Leto, no. 29; Leucippidae, no. 31 (uncertain); Medea and Pelias, no. 32 (uncertain)) have no attached triglyph; two (Mourning Women, no. 22 and Herakles and Nemean Lion, no. 16) have the triglyph on the right. The blocks succeed each other above the wooden (?) architrave in a curious arrangement so that they overlap slightly, each triglyph on the preceding metope-border. They are also set at an angle to the front edge of the architrave, as it were *en échelon* (see 67, fig. 17). At each corner is a two-faced triglyph block. The triglyphs taper upwards slightly and the metopes downwards; the blocks vary slightly in thickness.

Apart from the cuttings on the inside and top of the slabs, which have to be fitted into the roof construction, the presence or absence of attached triglyphs, and their placing on the left or right, restrict the possible arrangements of the metopes in order. There are two further limiting factors: (a) it appears that when the building fell into disuse (and also suffered damage by fire) the metope-triglyph blocks were dismounted (the good state of preservation of some seems to indicate that they did not fall from a height) and, before their use elsewhere, were laid on the ground approximately at the same point (in relation to the side of the building) where they had been located above the architrave and some stayed thus—this seems clearly demonstrated in the case of the Tityos and Leto metope no. 29 by its distance from the corner triglyph (see 320). (b) Five blocks on the rear short side (Silens alarmed, no. 8; Silens



advancing, no. 9; Dioscuri, no. 30; Leucippidae, no. 31; Hero on Tortoise, no. 27) bear the mark H on the top edge, which the excavators interpret as *ἡσπερος*, i.e. 'west side'; correct enough if the building workers used the Achaean alphabet of Poseidonia, but not if they had an Ionian origin; Achaean H =  $\eta$ ; Ionian H =  $\eta$ . Taking account of these limitations the excavators have arranged the metopes (pl. XIX-XXII). They have also taken account of the indications given by their own interpretation and association of metopes together in groups, since episodes seem to be divided in some cases between several metopes, or successive metopes represent successive stages in the myth. They thus arrive at the following arrangement: front, l. to r.: Pholos, no. 1; Herakles Shooting, no. 2; Wounded Centaur, no. 3; Dying Centaur, no. 4; Galloping Centaur, no. 5; Galloping Centaur, no. 6; rear, Herakles and Hera, no. 7; Silens ('frightened'), no. 8; Silens ('running'), no. 9; Dioscuri, no. 30; Leucippidae, no. 31; Hero on Tortoise, no. 27; right side: Herakles and Antaios, no. 15; Herakles and Kerkopes, no. 13; Herakles and Deianeira, no. 10; Eurytion, no. 11; LOST; Theft of Tripod (Apollo and Herakles), no. 12; LOST; Herakles and Nemean Lion, no. 16; Medea and Pelias in Cauldron, no. 32; Daughters of Pelias, no. 33; Laodamia and Clytemnestra, no. 24; Orestes and Aegisthus, no. 25; left side: Orestes and Erinyes, no. 26; Apollo and Artemis shooting, no. 28; Tityos and Leto, no. 29; Mourning Women, no. 22; Prothesis Scene (Hecuba and Hector (?)), no. 23; Zeus, no. 19; Iris, no. 20; Death of Patroclus, no. 21; Achilles in Ambush, no. 18; LOST; Herakles and Nessos, no. 17; Herakles, Erymanthian Boar and Eurystheus, no. 14. There are, according to this arrangement, three lost metopes (two on right side, one on left): one of these, of which there are surviving fragments, may represent Herakles and the Cretan Bull, or Europa and the Bull.

A natural criticism of this arrangement is the separation of scenes which one might expect to be grouped together: most of Herakles' adventures are on one side, but two are on the other; Orestes and the Erinyes are separated by the six back metopes from the rest of the Oresteia scenes. It is, indeed, possible to fill in the gaps on the right side with Herakles and Nessos (after Eurytion), and Herakles, the Boar, and Eurystheus (after the Theft of the Tripod), taking these from the left side; also, by leaving a blank metope after Herakles and the Nemean Lion, Laodamia and Clytemnestra and Orestes and Aegisthus can be moved forward, leaving a space before the corner triglyph for Orestes and Erinyes from the last place on the left side. Then Medea and Pelias (which may or may not have had an attached triglyph) and the Daughters of Pelias can be transferred to the other (left) side in the first two places, leaving a space for a metope with triglyph after Achilles in Ambush, i.e. for Troilos. The last metope on the right side is then left empty, while after Herakles and the Nemean Lion there would be place for Herakles and the Bull as a metope without triglyph (which it might be). This re-arrangement may, however, be excluded by the position of the cuttings on the rear and top surface of the slabs.

A great deal of the above depends, of course, on the interpretation of the subjects of the metopes. The interpretation of five metopes, nos. 1, 3-6, and their association together on the front, as a representation of Herakles' visit to Pholos and its sequel, are eminently reasonable: and the very battered Herakles Shooting, no. 2, will fit in here. On the rear, if we accept the significance of the marking H, five metopes go together, and, given the Silens, the interpretation of no. 7 as Herakles defending Hera from their attack is again a reasonable one (especially as Herakles here figures so prominently any way in a *temenos* of Hera). On the same side the Hero on the Tortoise stands apart; the pursuit of the Leucippidae by the Dioscuri is a reasonable interpretation of pursuers and pursued on the remaining metopes. Of the adventures of Herakles on the long sides some are quite clear, so the Kerkopes, the Theft of the Tripod, the Nemean Lion, the Boar and Eurystheus; less so is Antaios, and quite uncertain are Deianeira, Herakles, and Eurytion (this combination of two metopes is very doubtful), and Herakles and Nessos (on account of the damage to the slab). The Pelias and the Orestes sequences are also dubious, as is the Trojan series (except Achilles in Ambush): Zeus might be Priam, but too little remains; Apollo, Artemis, Tityos, and Leto are none too certain. But this is not to say that the excavators have not displayed extraordinary skill in the combination of metopes to find a suitable interpretation (cf. especially Orestes and Erinyes), often having to deal with small fragments or greatly damaged blocks, and producing a wealth of comparative material (cf. the interpretation of the Hero on the Tortoise). A good deal is clearly owed to Kunze's *Schildhänder* (see 8, Prefazione). They make it quite clear that they feel full confidence only in the arrangement of the six metopes on the front.

Anyone seeing these plates or the splendid reconstruction in the new museum at Paestum must be struck by two features of the metopes, (i) the different states of completion of the metopes and (ii) the oddly heterogeneous scenes represented. He will also have been greatly puzzled by the problem of dating, made no easier by the wide divergence of opinion on other Western buildings and sculpture, especially Temple C at Selinus, as the authors of this work point out in Vol. II (194, n. 4, and 194-5 *passim*), and in Vol. I.

The unfinished state of some of the metopes (Herakles and Antaios and the fragmentary head of a Mourning Woman, from the 'prothesis' scene, represent the earliest state of cutting out the chief planes against the background, while one of the Fighting Centaur slabs, no. 5, the Silens, Mourning Women, Laodamia and Clytemnestra, Orestes and Erinyes, Apollo and Artemis Shooting, and Tityos and Leto, represent a further stage of elaboration of detail and subsidiary planes) will provide a mine of information on the use of tools, especially the punch, and technique in general. The authors, indeed, suggest the use of lead profile models for reproduction of more or less identical outlines (76-8). But there is the problem of why they are unfinished. Some metopes, it is said, were finished before being placed in position (73), though even some of these show tool marks. Others were not. Can we be quite sure that the unfinished were in fact placed in position, or that any were placed in position, especially since so little evidence remains of the roof? It is true that there are signs of adjustment of the overlap of triglyphs over the frame of the preceding metopes, but this could have been tried out on the ground. The decisive evidence seems to be the cuttings for roof-beam ends and rafters in the top and back of some of the slabs; these could hardly have been cut without trial and error tested *in situ*. The excavators are therefore forced to propound the ingenious theory that the sculptors failed to complete all the metopes in time on the ground, and so, finished or unfinished, they had to be hoisted into position to allow the roof to be completed as a protection to the rest of the building (see 71, 72, 73-5). There is here a suggestion of inexperience resulting in a failure to allow a sufficient margin of time, which is reinforced by the odd placing of the blocks and the need for adjustment of the overlap.

But the question still remains why they were not completed *in situ*. The natural explanation is the cutting off of finances or the removal of the authority which had set in train the construction of the building. The need to discover such a set of circumstances caused the excavators to connect the building with the city of Siris in South Italy (9-10, 105-6). They stress the mixed style of the sculptors employed (reflecting Attic, Ionic, and Peloponnesian details, but not clearly related to any one) and conclude: 'Sicché ci risolviamo a datare le sculture nel secondo quarto del VI secolo, fra il 570 e il 560, ed attribuirle ad artisti operanti senza dubbio sul luogo ed ammaestrati probabilmente nella città di Siris e sotto l'influsso della sua raffinatissima civiltà, la cui morte violenta riteniamo causa dell' incompiutezza' (106).

This ingenious suggestion, which can be subjected to a number of criticisms arising from our limited knowledge of the history of Siris, gives in fact no terminus *ante quem* (see 9) or period for the sculptures (independently of their style, which is not easy to judge), and does not get us very far: the sculptures were left in their present state of finish sometime between 582 and 550 B.C.! The excavators detect three sculptors as participating. The Centaur Master (nos. 5, 6, 14, 21, and probably 19 and 20), the Pholos Master (nos. 1, 3, 31, 33, probably 2, 4, 30, and 32), and the Tripod Master (nos. 7, 8, 9, 10, 11, 12, 24, 25, perhaps 13 and 27). The surface even of those metopes which are most highly finished and most significant for style (Pholos, Galloping Centaur, Eurytion, Theft of Tripod, Herakles and Kerkopes, Herakles and Boar, Orestes and Aegisthus, Hero on Tortoise, Dioscuri, and Leucippidae) is poor and generally damaged, and it is not surprising that the stress in the publication is laid on 'volume' and disdain of linear detail (98). The excavators also detect (100) an archaic treatment of the heads, and 'greater interest' of the artists in the bodies. It is true that the huge heads and enormous eyes never properly sunk in their sockets give, rather than the ill-formed lips, an archaic impression, reinforced by the rendering of the male body which leaves no space for abdomen between chest and thighs. Yet though there is a good deal of (primitive) frontality, facing heads are rare (Mourning Women only). It is doubtful if as much can be made of muscle rendering (in comparison with vase painting) and other details of anatomy as the authors attempt to do, though here and there the efforts to conceal the half-frontal, half-profile shoulder rendering is noteworthy: cf. Apollo, pl. LXVI. Little can be made of the limited renderings of drapery (best seen in Herakles and Deianeira and Leucippidae metopes). Yet in noteworthy contrast to this 'archaism' is the degree of undercutting and



separation of heads from the background in the latter metope (345). Out of keeping with all the inferior and seeming archaic execution is the lively spirit and bold combinations of figures in some of the slabs (Gallopings Centaur, no. 6; Orestes and Aegistheus; Hero on the Tortoise; Herakles and the Kerkopes; Dying Centaur, no. 3; Leucippidae), as if rather unskilled sculptors were working with a set of patterns (of different dates) drawn from some more developed centre (which the authors are in some fashion forced, though unwillingly and fleetingly, to suggest (105): 'Un arte troppo esperta e raffinata per appartenere ancora alla prima metà del VI secolo')—an impression reinforced by the evidence in the construction of the building of no first-class skill. In this case the date of the sculpture could be later than the second quarter of the sixth century, and the connection with Siris, as the authors see it, falls to the ground. Parallels in other sculpture are not easy to find: the closest in general impression is the Sphinx from Selinus (*Kunstgeschichte in Bildern* I 7, 199, 3); the finer detail of *ibid.* 2 (Europa) seems quite alien to the Sele metopes. The temple C metopes seem more advanced (especially the heads—of different dates (?), *ibid.* 7, 8), but here material and preservation count for much. Kunze (*Schildbänder, passim*) seems to regard the Sele metopes as of mid-sixth-century date, but sees the Kerkopes metope as earlier than the rendering of the same subject on temple C at Selinus (which is of about the middle of the sixth century). The dating is likely to be a problem, but it is almost certainly later than that given by Zanotti-Bianco, and if we must connect Siris and its destruction, then that destruction must be later than our authorities infer.

There remains the problem of the choice of subjects for the metopes. One is almost forced to think of the use of a job-lot of assorted patterns, of mixed origins like the parallels so abundantly provided by the excavators. To explain this heterogeneous collection they suggest the works of Stesichoros as a possible source of inspiration (106-7), the *Geryoneis*, *Helen*, *Iliupersis*, *Nostoi* and *Oresteia*. A brilliant idea difficult to prove conclusively, and as difficult to disprove. But some such explanation is needed.

The excavators are full of enthusiasm for their discoveries. Possibly they detect too many excellences in the sculpture, and discuss everything at too great length; there is a certain amount of repetition (on Treasury I) which might have been avoided. The discussion of parallels in Vol. II is outstandingly valuable (e.g. Orestes and Erinys or the Hero on the Tortoise), but in view of the cost of modern book-production the whole is perhaps too long and conceived on too lavish a scale. None the less, the aim of the work is clearly to provide the reader with all he needs to know to reach an independent judgement, and no one would criticise the abundance of splendid drawings and (for the most part) excellent plates. The photographs sometimes leave something to be desired, and those in the text are sometimes better than the plates, contrast II, fig. 84 and II, pl. XCIV, but nothing could be better than the photographs of the same temple metope (in Vol. I) taken from different angles. Printing and paper and editing seem excellent; there are sometimes trifling errors in Greek quotations. The drawings need captions and the plates metope numbers. Peculiarly irritating is the deficient system of numbering for cross-reference between text and illustration. Yet these are trifles in such a superbly produced work.

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**Greek Sculptors at Work.** By C. BLÜMEL. Trans. by L. HOLLAND. Pp. 85, with 68 text figures. London: Phaidon Press, 1955. 25s.

The first German edition of this valuable work appeared in 1927 under the title of *Griechische Bildhauerarbeit*, in the 11th Ergänzungsheft of the *Jahrbuch des Deutschen Archäologischen Instituts*. It was followed by a more popular version entitled *Griechische Bildhauer an der Arbeit*, which went through three editions. The present book is an English translation of this popular version. It differs from the first, larger, scientific publication in that it omits footnotes, bibliography, and index; moreover, though it presents most of the original illustrations (sixty-eight out of eighty-five), it has no longer detailed descriptions of the pieces illustrated.

The great merit of Blümel's work has been to assemble excellent illustrations of many unfinished statues and reliefs and to interpret the technique of Greek sculptors in the light of this revealing evidence. He was the first to do so systematically; for his illustrious predecessor, Hugo Blumner, in the third volume of his *Technologie und Terminologie*, 1884, pp. 187-226, gave few illustrations, and E. A. Gardner's article on the 'Processes of Greek Sculpture as shown by some unfinished statues in Athens', in the *JHS* XI (1890), pp. 129 ff., was a mere sketch. Blümel, on the other hand, supplied a wealth of

illustrations of sculptures ranging from the archaic period to Roman times, and convincingly showed the different approaches of the Greek artists and of the copyists of the Roman age. Interest in this subject was thereby thoroughly aroused, as shown by Stanley Casson's fine book on *The Technique of Early Greek Sculpture*, which appeared in 1933, and which was followed by many subsequent researches.

The great defect of the 'popular' version of Blümel's book is that it treats highly controversial questions as settled, and gives the reader only one answer, namely the author's. We have here a perfect illustration of how misleading can be the omission of footnotes even in a popular volume. I may give a few instances.

The important problem regarding the nature of the models used by the Greek sculptor is settled by the definite statements (pp. 42 ff.) that full-size clay models were made as early as the time of the Olympia pediments, that parts of these models were then fashioned in stucco and plaster, and that from these parts full-size plaster casts were made for use by the sculptors. That we have no evidence for full-size plaster casts of statues until considerably later is not mentioned. Instead, it is asserted that plaster would have been the only appropriate material for models, even in early Greek times, for a detailed finish 'can only be obtained by working on a plaster mould'. Clay, it is thought, would have been too soft for retouching, since 'it is sensitive to the lightest pressure of fingers' (p. 43). But surely clay in leather-hard condition has just the right consistency for retouching, as is indicated by Greek vases and some terracotta statuettes and reliefs.

In this connection Blümel cites the building inscription of Epidauros, in which Timotheos is mentioned as having contracted to make and finish *ῥῆμοι* for 900 drachmas, and argues that these *ῥῆμοι*—which he takes to mean models—must have been full-size, since in Timotheos' time 900 drachmas represented the wages for 900 working days. There is no mention that some authorities think that *ῥῆμοι* means 'reliefs' (cf. the references cited in my *Sculpture and Sculptors of the Greeks* (1950), pp. 277 f.). In fact, the high sum paid for these *ῥῆμοι* would support that translation, and so does not necessarily supply evidence for full-size models.

That this one-sided presentation is not Blümel's fault, but is due to the exigencies of a popular presentation, is shown by the treatment of the subject in the 1927 edition (p. 24). Here it is admitted that the answers to the question 'sind so verschieden wie denkbar ausgefallen', and the problem is then presented in a fair way. In any case, whatever may have been the early Greek practice, it is well to remember that small wax models used by Michelangelo for his large sculptures are still preserved.

The difficult question of what method or methods were used by the copyists of Roman times is answered by Blümel by the statement that they employed throughout a scaled frame over the plaster model and marble block, from which corresponding measurements were taken by means of plumb lines (p. 57, fig. 42). The 'puntelli' visible on some ancient statues he thinks prove this, because they are not in the trio formation that would indicate the use of the 'Punktierzirkel', or of a pointing machine (which are based on the geometric principle that any three points can be viewed as being situated in one two-dimensional plane, whence the positions of other points can be ascertained). That, however, puntelli in trio formation exist on ancient statues I have tried to show in my *Ancient Italy*. In my opinion, therefore, the scant evidence we have for the procedure of the Roman copyists admits of several interpretations.

In still another question the popular edition of Blümel's book does him scant justice. In his first edition he enunciated his theory that the Hermes at Olympia was a Roman copy and not an original work by the great Praxiteles with many footnotes citing arguments on both sides. Though consensus of opinion on this question has not been reached, in Blümel's English edition the whole debate is settled very simply by the categorical statement (p. 85) that 'technical details on the unfinished reverse side' have shown 'that it [the statue] could not have been made before the end of the 2nd century B.C.'. Again, the *Ersatzfiguren* of the Olympia pediments—for which the assignments have ranged from the second half of the fifth century B.C. to Roman times—are simply said to be copies 'made in the days of imperial Rome'. It would seem that technical evidence admits of different interpretations, just as does stylistic evidence, but Blümel's 'popular' text envisages no problems.

To translate an intricate German text into English is, as we all know, a difficult task. The present translation is competent, though sometimes the idiomatic German has been misunderstood; as, for instance, when in the discussion of a gem in the Gotha collection (p. 48, fig. 35) *jüngere Gestalt* is translated 'younger figure' (younger than what, one might ask?).

The English edition is larger in format than the popular German one, and the illustrations are of better quality. And



that is important, for the illustrations remain constant, regardless of the different theories that may be held regarding them. We owe the Phaidon Press a debt of gratitude for presenting them adequately to the English public.

GISELA M. A. RICHTER.

**Manuel d'Archéologie grecque [IV] : la Sculpture IV ; Période classique—IV<sup>e</sup> siècle, deuxième partie, first part.** By CH. PICARD. Pp. 421; pl. 10, 177 text figs. Paris, A. & J. Picard, 1954. 2850 fr.

This volume will have entered most libraries as IV ii of the series and taken its place, to the dismay of librarians, next to III i 2. They are continuous. 'Deuxième partie' means that IV is the second part of III (just as on the title page of III 'première partie' proves to have meant that III was, so to speak, the first part of IV); it is therefore logical that IV (ii) 1 should succeed III (i) 2. This 'handbook' has not hitherto been introduced to readers of this *Journal*. It consists up to date of four tomes, of which two—I archaic sculpture, II (in two volumes) the fifth century—had appeared before the war. III (in two volumes) appeared in 1948 and covered the early fourth century, the youth of Praxiteles and the statues of Scopas. IV i (the volume here reviewed) is of extraordinary interest, since in winding up the account of the two sculptors it handles at length the majority of the great controversial topics of the fourth century. IV 2 will cover Lysippus, Leochares, and Bryaxis, anonymous works, and relief sculpture. Hellenistic sculpture is to be dealt with in the two concluding volumes of the series. M. Picard's treatment of this vast subject is discursive, his exposition is clear, consistent, and persuasive; and his discussion of controversial issues is especially valuable because of his balanced summaries of the progress of criticism and his analyses of conflicting views. He does justice to the spiritual qualities and does not over-estimate the importance of technical considerations. He is in fact uniquely qualified to be the author of this great work. The few collotype plates are beautifully done. The half-tones in the text are not. A few of the good British Museum photographs have been respectably reproduced, but the old black-background favourites, faded or taken second-hand from books, are no longer what they were in Furtwängler's day; and the block-makers seem to have lost heart. Even with these handicaps, why should drawings that were black and white in other people's books be reproduced here in a muddy grey on grey?

The volume begins with the Mausoleum. M. Picard reviews the previous attempts to distribute the sculptural decoration and to assign what survives to the masters named in the sources. He holds to the common view that the three consecutive slabs (1013-15) are Scopas', claiming that the resemblances in attitude to Tegea should count for more than the dissimilarity of the heads; in the meantime this position has been undermined by Ashmole's article in *JHS* 1951, and Leochares' claim will need to be considered in IV 2. Buschor's recent theory of interruption and a resumption of work on the Mausoleum sculptures after Alexander's capture of Halicarnassus is fairly considered and firmly rejected. M. Picard's most spectacular discovery is that of the hitherto obscure 'ensamblé' Satyros. Taking the view that the four eminent outsiders (Timotheos, Scopas, Bryaxis, and Leochares) were summoned to Halicarnassus to make the reliefs only, M. Picard assigns to Satyros the free-standing sculptures, including the Hecatomnid portraits and the splendid head of Apollo and Persian rider. Thus the four sculptors will have played only a secondary part in the sculptural decoration of the monument, the dominant role being assumed by Satyros, who is presented by M. Picard as court-sculptor of the Hecatomnids, Scopas' colleague at Tegea and author of the Sarcophagus of the Mourning Women from Sidon. M. Picard then proceeds to Ephesus. He would like to see the well-known 'Alcestis' drum as the one made by Scopas, and he takes the action on it to be the Sacrifice of Iphigeneia; this interpretation is appropriate at the Artemisium, but it seems to founder on the Angel of Death, who has no place in the iconography of this theme. Turning to the Artemisium gable, M. Picard nails his colours to Mrs. Trell's kite; from the conflation of faint and contradictory indications on late coins a tableau is recovered in which the wounded Amazons knock for admittance at the triple propylon of the sanctuary. Here M. Picard would like to locate the type of the Amazons of the fifth-century competition rejuvenated by the down-dated Phradmon!

M. Picard insists with good sense on Tegea being later than the Mausoleum. Unfortunately he has been misled by a confused memory of Pausanias' words (p. 166 top) into upsetting the orthodox arrangement of the central figures of the east gable and placing Atalante and Meleager on either side of the centre; on the strength of this blunder he reaffirms his attribu-

tion of a head in Dolianà marble to Atalante and considers the torso (called 'Atalante') in Parian marble to be irrevocably expelled from the pediment; M. Picard's argument in fact falls to the ground, since he has Atalante facing the wrong way, but the objection of the material still remains. He wishes to assign this torso, together with the 'Hygieia' head, to the federal altar, which was adorned with sculptures of nymphs framing a scene of the Birth of Zeus. Claiming (p. 203) that Pausanias says the back of the altar was decorated with figures of the Muses and Mnemosyne, M. Picard attempts a ten-a-side arrangement of these sculptures resembling the Sarcophagus of the Mourning Women; but Pausanias never mentioned the back of the altar. The point is not unimportant, since M. Picard attributes the design and execution of the altar not to Scopas but to Satyros himself, and this resemblance to the sarcophagus is the essential link in the chain of the Satyric oeuvre. It would be prudent to recall that all we know of Satyros' work is that he is named by Vitruvius as co-architect with Pytheas(?) of the Mausoleum and that he signed a base at Delphi which bore bronze statues of Idrieus and his wife: all else is remote conjecture. Mausolus had plenty of work for his architects to do, not only in Halicarnassus itself but at the other cities that he was founding on a Hellenic plan and scale; caution demands that in the present state of our knowledge we should be prepared to view Satyros rather as a successful engineer and undertaker of works for the Hecatomnids than as one of the giants of fourth-century art.

Turning back to Praxiteles, M. Picard takes up his work after his sojourn in Asia, whence the sculptor will have returned in the middle of the fourth century, ageing and grown respectable, to create a 'chaster, more veiled Olympus'. M. Picard accepts the Diana of Gabii as a reduced copy of the Brauronia, the tree suppressed in the copy; the gesture of adjusting the dress (which in fact could be seen on the Parthenon frieze nearby) is explained as symbolic of a novel relationship between Artemis and her worshippers. To the Hermes at Olympia M. Picard devotes nearly sixty pages. He refuses to accept it as genuine, mainly on grounds of general probability but also on account of the 'porcelain polish' and the present state of the back. Rejecting Blümel's heresy that its author was another sculptor of the same name, he takes the existing statue to be an exceptionally fine copy set up in place of the original, and is thus still able to use it as evidence of Praxiteles' art. But to avoid giving the lie to Pausanias M. Picard resorts to a mean evasion: when he said of the Hermes *τήν δὲ ἔκ τῶν Προξίτηδος* Pausanias will not have meant that it was a work of Praxiteles, but in his manner. By this criterion the great Bronze Athena of the Acropolis would be lost to Phidias, and the tyrannicides to Critios on the testimony of Pausanias; but in fact Paus. IX 30, 1 shows this argument false (*Κηφισοδότου τήν τε . . . τρεῖς μὲν εἰς αὐτῆς Κηφισοδότου*). M. Picard produces strong arguments against a late dating of the original, comparing the face of the infant Dionysus with Cephisodotus' Ploutos, and decisively rejects the theory of an original in bronze. The horizontal strut is condemned (though the local Hellenistic statuette of the Cnidia with their structurally unnecessary struts indicate that this precaution was used in the most famous of Praxiteles' works); on the authenticity of Praxitelian tree-trunks some discussion might be saved by reference to the Sauroctonus, who must otherwise hold his lizard on a string. The drapery emerges justified. M. Picard doesn't believe that the Hermes commemorated the pact of 343 B.C., but prefers now to see in the group an allegory of the eternal soul, influenced by Plato's myths, the ancestor of the butterfly-winged infant Dionysus of a mosaic at Antioch. After the Hermes, M. Picard appraises the statues of Dionysus and Apollo which (in general following Rizzo) he recognises as Praxitelian and assigns to the maturity of the master. The Mantinea base, which he prefers to attribute to the sons of Praxiteles, leads on to qualified attributions of the ladies with cantaloup-coiffures and the Eleusinians. M. Picard sees a mixed aesthetic in the Aberdeen head, but does not doubt that the rustic mysticism of the Eubouleus with its 'semi-fluid, almost equivocal planes' is a creation of Praxiteles in his last years. According to M. Picard the originality of Praxiteles is not to be sought in his sculptural forms and poses, which are rooted in Attic tradition, but in the new spirituality of his work; there is justice in his vindication of the religious sincerity of the second classicism.

For M. Picard the careers of both Scopas and Praxiteles are articulated by a triumphal peregrination in the sculptor's prime; and we are again and again told that the sculptor cannot have made a certain work at a certain time because he was then voyaging in the East. What are these Odysseys? Praxiteles, we learn, left Athens about 364 B.C. for Cos and Cnidus, to make the Aphrodites there; soon after the conflagration of the Artemisium in 356 he was working at Ephesus; 'ensuite, remontant vers le Nord il aurait créé l'Éros de Parion, en



Mysie. Sans doute revint-il par le Bosphore; quoique marins en principe les Grecs économisaient volontiers les navigations longues (III 2, 558). During this period of ten years and upwards Praxiteles will never have set foot in Greece! So too Scopas, if he worked in Thasos, will have done so on his way back from the East! But this is to treat Scopas and Praxiteles like travelling lecturers or musicians. If we turn to that contemporary by whom (in M. Picard's view) Praxiteles was most profoundly influenced, we find that Plato, having work to do, made the longer and more hazardous journey to Sicily no fewer than three times. One asks oneself also, can Praxiteles really have been occupied for the best part of ten years in making two life-size statues for the Triopian Dorians? But in any case what right have we to suppose that Praxiteles made the two Aphrodites in the cities to which he sold them? The Hellenistic tradition, which must be true to current practice, shows these two statues as already completed in the sculptor's own studio. Intuition adrift from the available evidence is a dangerous instrument; and it is largely through lack of consideration for the ancient sources that M. Picard goes off the rails on the main issues of fourth-century sculpture. This manual in consequence is not one to be handled without considerable reserve. Nevertheless, it is of first-class importance; and scholars will derive great benefit from studying M. Picard's work, with his admirable interpretation of the surviving sculptures, his masterly grasp of the relevant material and analysis of controversies, his insistence on the significance of symbolical and mystical values, and his sympathetic understanding of the whole range of hopes, fears, and beliefs in the ancient world.

J. M. COOK.

**The Sculpture of the Hellenistic Age.** By M. BIEBER. Pp. xi + 232, with 712 plates. New York: Columbia University Press, 1955 (London: Geoffrey Cumberlege). £7.

Dr. Bieber writes as an enthusiast; and it is indeed refreshing to find a classical scholar who is truly appreciative of Hellenistic art. It is still the prevailing fashion to dismiss it as 'theatrical' or merely 'prettified', at its best, as decadent or even downright bad, at its lowest. But the author of this substantial and finely produced book concludes that 'this period is one of the most important in the history of art. Nothing was lost of whatever previous periods had achieved: neither the charm and freshness of archaic art nor the grandeur and harmony of classical art was sacrificed by Hellenistic artists. On the contrary, infinitely more was added. . . . Man, in Hellenistic sculpture, is seen against his background and among his surroundings.'

A full-dress and richly illustrated study in English of Hellenistic sculpture was certainly due. G. Dickins' *Hellenistic Sculpture* (1920) and A. W. Lawrence's *Later Greek Sculpture and its Influence on East and West* (1927) were pioneer essays in this field, but are now out of date, to some extent at least. The second and third chapters of G. M. A. Richter's *Three Critical Periods in Greek Sculpture* (1951) are confined to a general discussion of specific questions. Again, both Professor Lawrence and Dr. Richter end by shepherding their readers into what are, strictly speaking, Roman or Graeco-Roman pastures. B. justly remarks that 'the borderline between late Hellenistic and Graeco-Roman art is very difficult to draw'; and that 'Roman art emerged as an unbroken continuation of Greek art'. Nevertheless, she delimits her own province, both in time and space, on clear and fixed principles. At one end, the first three quarters of the fourth century are excluded from her period as belonging to the 'classical', Hellenic age: the Hellenistic age began with Alexander in politics and with Lysippos in sculpture. At the other end, she comes to a halt at the Battle of Actium, when the ancient world was at last united under the rule of Rome, and Greek art-traditions and artists entered imperial service. Geographically B. confines her subject to the eastern Mediterranean world—to Greece proper, Asia Minor, the Aegean Islands, Syria, and Egypt (the last being extended to include, very cursorily, Cyrene). The impact of Hellenistic art on Etruria and the rise of the Pasticlean school in Magna Graecia during the last century B.C. are both omitted: Arkesilaos is the only western Greek sculptor to receive a passing mention. This leaves the Hellenistic scene incomplete. But the excursions of classical art into the Far East, to Gandhara and still farther afield, are rightly ignored, since we now know them to be part of the story, not of Hellenism, but of 'Rome beyond the imperial frontiers'.

A brief introductory chapter outlines the salient characteristics of Hellenistic art and blocks it out into three main epochs—(1) c. 330–250; (2) c. 250–160; (3) c. 160–30. In the first epoch the prevailing spirits are Lysippos (master of the third dimension) and Praxiteles; Athens leads; while at Alexandria

and elsewhere a softened Praxitelean style appears. The second epoch saw the blooming of 'baroque' in many quarters of the Greek world, most brilliantly at Pergamon, and the flowering of 'rococo', most gaily at Alexandria and in Asia Minor. During the third epoch both 'baroque' and 'rococo' remain; while a strongly classicising movement is staged on the Greek mainland. Most properly, B. encourages us to think throughout in terms, not of local, 'national' schools, but of great art-centres, the political and social life of which naturally coloured the work produced within their orbits, but which attracted to themselves cosmopolitan groups of sculptors drawn from many diverse 'nationalities'.

A rapid, but skilful, sketch of fourth-century Greek sculpture links Hellenistic artists with their fifth-century, and ultimately with their archaic, forerunners. Chapter III, of which the hero is Lysippos, deals first with well-known statues of men, gods, and personifications ascribed to this master and his pupils, and secondly with portraits. Here, at the outset, a criticism must be made which applies throughout the volume: there is everywhere a tendency to equate extant sculptures somewhat too easily and glibly with famous works recorded in our literary sources. For instance, the 'Maiden of Anzio' in the Terme is said to have been recognised as the 'Sacrificing Woman' by Phanis. No detailed evidence supporting this equation is given; but the excellent description of the statue ends with its categorical classification as 'an early Hellenistic masterpiece'. An original it undoubtedly is. But in view of the very eclectic nature of late-Hellenistic and Roman taste, we cannot be sure that it was carved so early. In portraiture, in particular, Lysippos is justly claimed to have marked a turning-point, although his late-fifth- to early-fourth-century predecessor, Demetrios of Alopeke, had already blazed the trail, if the literary records of him can be credited. But to what extent his portraits were truly realistic, and not just highly naturalistic, it is hard to say. At any rate, we can hardly ascribe to the hand of that Hellenic master the intensely 'veristic' marble head of an old woman in the British Museum, tentatively identified, as B. reminds us, with his likeness of Lysimache, the aged priestess of Athene. This is a first-century B.C. face, Roman or late-Hellenistic, combined with a pseudo fifth-century, consciously archaising hair-style. From the portrait-types of Plato and Sokrates we pass to the Lysippan Alexander and thence to the portraits of the early Diadochoi. We can be reasonably sure that the bronze bust from Herculaneum in Naples (figs. 141–3) depicts Seleukos I, on the score of its likeness to his coin-portraits (fig. 140). But B. does not point to the remarkable contrast between the die-sinker's ruthless realism and the 'toned-down', idealised features which the bronze-worker presents to us. Hellenistic men-of-letters and philosophers complete the portrait-series; and here, inevitably, we find ourselves involved once more in the burning 'Menander-Virgil' controversy, in which B.'s own position as a staunch and unrepentant 'Menanderite' is widely known. The arguments used by her 'Virgilian' opponents—Crome, Lippold, Rhys Carpenter, and Herbig—are, to my mind, scouted far too airily. No reference is given to Rhys Carpenter's minute analysis (*Hesperia* XX, 1951, 34 ff.) of the many significant divergencies between the disputed head and the Marbury Hall inscribed Menander 'medallion', on which the whole Menander case hinged; or to the points which have led G. Hafner (*Späthellenistische Bildnisplastik*, 1954) to conclude that this head is certainly not that of Menander, but is a Greek portrait of the first century B.C., which might be that of Virgil, taken when the poet was in Athens.

In Chapter IV a galaxy of statues of famous men—of the three great tragedians, of Aristotle, and of Aeschines—are assigned, somewhat vaguely, to Atticism of the late-fourth and early-third centuries. The Belvedere Apollo and Artemis of Versailles are associated with Leochares, for no other reason, so far as one can see, than that their composition is reminiscent of that of the sculptor's well-known Ganymede and Eagle. Much more certain as works of this Attic group are the Themis from Rhamnos and the seated Dionysos of Thrasyllos. With Polyektos' closely dated Demosthenes we reach firm ground; and we extend a special welcome to the four fine views of the little-known bronze statuette in the Strauss-Hess Collection in New York (figs. 226–9), where the correct position of the orator's arms, with folded hands, is excellently recorded. Portraits of prominent Stoic and Epicurean philosophers round off this chapter.

Under 'Asianism in the Third Century B.C.' (Chapter V) B. considers the achievement of Greek sculptors in the service of non-Greek Asiatics, the development of groups (Niobids, Menelaos, and Patroklos, etc.), and the activity of Greek artists in the service of Hellenistic rulers—including the work of Boethos, Doidalsas, and the Younger Bryaxis. In this last section B. does ample justice to the splendid series of vitally



'veristic' royal coin-portraits, in which the personalities and facial oddities of monarchs of Egypt, Syria, Asia Minor, and Bactria have been preserved to us. But the heads of Serapis which she reproduces (figs. 291, 297, 388) are not of Bryaxis' type of the first half of the third century B.C. They are all of the Roman type (with four locks dangling on the brow) which, to judge from the existing replicas, originated in the second half of the second century A.D. If, as seems probable, this type was created for the recently discovered Roman Serapeum at Alexandria, it would date either from the reign of Hadrian or from that of Commodus. The Ptolemaic temple may well have been destroyed during the Jewish insurrection under Trajan and replaced by Hadrian. Alternatively, Commodus may have been the author of the new Roman shrine, since we know that in his time the Alexandrian Serapeum was severely damaged by fire.

Chapter VI takes us to Alexandria, the cosmopolitan centre *par excellence*, where artists from all parts flocked together. It may well be that works in the 'melting' Praxitelean manner originated there: Alexandria's abundant supplies of stucco as a medium for sculpture were favourable to such a style. From the likenesses of Ptolemies and their queens we can trace in part the story of Alexandrian iconography, but a story that is here, unfortunately, left unfinished, since B. says nothing of the important series of late-Ptolemaic portrait-statues of Egyptian priests, etc. (of which the Berlin Harsinebef is a striking instance), with conventional bodies acting as foils to astonishingly realistic heads, which anticipate the work of first-century A.C. Roman republican portraitists (*AJA*, LVIII 1954, pp. 143-4). We need not doubt that statues of specifically Egyptian deities were carved in Alexandria; and the same can be said of specifically Egyptian figures, groups, and scenes drawn from the worlds of daily life and scholarship. Many such works have come to light in or near the city. But to class all *genre* works under the heading 'Alexandria', as B. does, is surely misleading. In the Hellenistic period renderings of children, rustics, drunken females, dwarfs, and other abnormalities catered for a very widespread, 'international' taste. A fancy even for negroes and Ethiopians was no monopoly of dwellers on the Nile delta. Two papers by A. J. B. Wace, 'Apollo on the Omphalos' (*BSA* IX, 1902-3, 211 ff.) and 'An Altar from the Serapeum' (*Bull. Soc. Roy. d'Arch. d'Alexandrie* XXXVI, 1943-4 (1946) 83 ff.), neither of which appears in B.'s bibliography, should be read by all students of this chapter.

Chapter VII summarises, in a trio of pages, the sculptural harvest from the excavations at Priene. In Chapter VIII, 'The Art of Pergamon', B.'s sympathetic pen has given us what is by far the most balanced, comprehending, and unprejudiced appreciation of sculpture under the Attalids that has yet appeared in the English language. The essential grandeur of the great Gigantomachy reliefs, the originality of their conception, the mastery of detail and brilliance of technique displayed by their carvers—these are qualities, emphasised by B., that cannot be questioned by any observer who studies the altar without preconceptions. Valuable, too, is B.'s running commentary on the less familiar Telephos frieze, 'where for the first time in art history a genuine narrative style is used'—the true precursor, so she might have added, of the Roman 'continuous' method of narration which was developed so successfully, both in relief and painting, under the late Republic and Empire. But it is strange that B. never so much as hints at the existence of the lovely floral scrolls (frieze-fragments) and fruit-and-flower garlands (round altar of Eumenes II), that distinguished the decorative art of regal Pergamon and were destined to inspire directly the designer and executors of the Ara Pacis Augustae (T. Kraus, *Die Ranken der Ara Pacis*, 1953).

Uncertainties thicken when we turn to the work of sculptors active in Rhodes and in south-west Asia Minor (Chapter IX). Fixed points are Chares of Lindos' Colossus, and works of local provenance, such as the Tozzi Collection Hermaphrodite (fig. 492), the tomb-relief of Hieronymos the schoolmaster (fig. 491), both found on Rhodes, and the well-known female torso with transparent himation from Magnesia on the Meander (fig. 520). This manneristic drapery-style also appears on the Telephos frieze from Pergamon: whether it actually originated within the Rhodian sphere of influence we do not know. B. classes as Rhodian reliefs the family sacrifice at Munich (fig. 489) and the Apotheosis of Homer, signed by Archelaos of Priene and now in the British Museum (fig. 497); but her reasons for doing so are tenuous in the extreme. Again, is it true to say that the Nike of Samothrace 'has now with certainty been ascribed to Pythokritos of Rhodes'? A marble fragment, inscribed . . . Σ ΠΟΔΙΟΣ, was found near the statue, *may* have belonged to it, and *may* have recorded its author's signature; and 'several inscriptions signed by Pythokritos of Rhodes have the same letters as the inscription in

Samothrace'. This is a basis for conjecture, reasonable perhaps, but it is not certainty. B. makes no allusion to K. Lehmann's excavations on the island, to his precious, because objective, ceramic evidence confirming the c. 200-190 dating by style (*Ill. Lond. News*, 25.xi.50, p. 867), to the finding of the Nike's right hand—open, raised, extended, and apparently empty, with palm turned outwards (*Hesperia* XXI, 1952, pls. 12, 13), or to the realisation that the goddess on her prow was the central motif in a ship-fountain, poised in a 'landscape' of natural boulders and artificial marble steps, and reflected in a basin into which an aqueduct brought water (*Archaeology* VI, 1, 1953, p. 35). Reference to this magnificently 'baroque' setting would have enhanced, to no small degree, B.'s fine appraisal of this masterpiece.

The Dirce group ('Farnese Bull') and the Laocöon are definitely Rhodian products. The first was brought to Rome from Rhodes, and was the work of two south-west Asiatic sculptors who were probably adopted by a Rhodian sculptor. The Laocöon, as we have it, is ascribed to three Rhodians, two of whose names occur in Rhodian inscriptions of the second half of the first century B.C.; and it is to this late date that B. assigns the piece. Yet its close kinship with Pergamene 'baroque' is obvious. Could not the Vatican marble be a late-Hellenistic copy, made by those Rhodian artists, of an early-to-mid-second-century bronze original?

'Rococo Trends' form the subject of Chapter X. In Hellenistic art 'rococo' did not follow, and react against, 'baroque', as in the eighteenth century: it had a continuous history throughout the greater part of our period. Its favourite materials were terracotta, bronze, and ivory, although it often used marble also; its favourite themes were children, adolescents, old men and women, Dionysos and his train, Aphrodite, Eros; and in this book, the term covers single figure, one-sided groups, pictorial reliefs, and representations of animals. Of reliefs with a continuous landscape or architectural background all are of the second half of the first century B.C. or later, when Rome dominated; B. does not suggest that this may be significant, indicative of a taste that was fully developed only in the West. It is somewhat odd to discover under the heading 'rococo' the powerfully 'veristic' and prosaic head of an ageing priest from the Athenian Agora (p. 142)—one of the best examples of a series of late-Hellenistic forerunners of those relentlessly realistic, almost 'photographic', likenesses of old persons in which artists of the late Republic specialised. The Naples bronze 'Seneca' (fig. 596) features here as 'probably an invented portrait of Aristophanes'—solely, it seems, on the ground that replicas of it are, in the case of two double herms, backed against the head of the so-called 'Menander'. But if 'Menander' is really Virgil, an appropriate companion would be a fellow Roman poet, Lucretius, as was proposed by G. della Valle in a paper which B. quotes in a note, but does not discuss. A 'fancy' portrait of Lucretius, done in the 'baroque' manner of the early to mid-second century B.C. as best expressing his 'furor arduus', fits the discovery of the Naples bronze in an Epicurean villa and the fact that nearly all the many versions of it are of Italian provenance.

The theme of Chapter XI is classicism in the second and first centuries before our era. A. W. Lawrence's note on 'cessavit ars' in *Mélanges Charles Picard*, 1949 deserves to have been quoted on p. 159. Again, it is a pity that B. should have ignored J. Charbonneau's valuable paper, in *Revue des Arts* I, 1951, on the Venus of Milo and the so-called 'Inopus'. Charbonneau makes an important point about the statue's proportions, to which the attention of students should be drawn. Furthermore, B. states that 'the goddess certainly rested her lost left arm on this pillar [assuming, what is far from certain, that the lost inscribed fragment of a base, with a pillar-socket in its upper surface, really belonged to her], for her left shoulder is raised'; whereas Charbonneau, as I believe rightly, arrives at the precisely opposite conclusion: the slope of the shoulders rules out a pillar, which must henceforth be banished from all reconstructions of the figure. It is also to be regretted that Lehmann's graceful, willowy, libation-pouring Nike from the Samothracian temple (*Hesperia* XX, 1951, pls. 15, 16), with 'Pheidias zig-zags' on her drapery, finds no place in this survey of classicising statuary. The late-second-century architectural friezes from Magnesia on the Meander and from Lagina in Caria would seem to be far less retrograde than B. believes: she does less than justice to the value of the three-dimensional landscape and perspectival effects in which their carvers experimented.

In Chapter XII, 'Conclusion', it is good to see Rome's services to Greek art generously recognised. The elementary fact that we owe the survival of most Greek masterpieces to 'classical' taste on the part of Roman connoisseurs and collectors is too often forgotten, as indignation burns at the looting propensities of conquering generals and art-loving emperors



and as scorn is poured out on the productions (some admittedly poor, others excellent in quality) of Roman-age copyists. But Roman taste was, all along, less exclusively 'classical', more eclectic and 'catholic', than B. intimates. The characteristically republican iconographic manner was derived from that of late-Hellenistic portraitists, as has been pointed out already; the rich, if controlled, exuberance of regal Pergamon's floral style lived again, as we have seen, on the walls of an Augustan monument; and for all we know, the great 'baroque' groups may have been conveyed to Rome several decades before the advent of the Flavian, pro-'baroque' period, when Pliny records their presence in the capital.

*The Sculpture of the Hellenistic Age* may provoke disagreement, or even disappointment, in matters of detail; but in general it offers a generous response to a long-felt need. Complete with Chronology, Selected Bibliography, Index, and List of Plates, it will long remain a standard source of information (so far as it goes) and delight to all who are attracted to the study of this vital period in the history of human culture. The plates are mostly of the first calibre, although a few are poor. Never before has the full range of Hellenistic sculpture been presented to the eye on so extensive a scale.

J. M. C. TOYNBEE.

**Greek Portraits. A study of their development.** (Collection Latomus, XX). By G. M. A. RICHTER. Pp. 50, with 10 plates. Brussels: Latomus, 1955. B.Fr. 80.

**Les portraits grecs (Publications de la Glyptothèque Ny Carlsberg, 5).** By V. POULSEN. Pp. 87, with 45 plates. Copenhagen: Ejnar Munksgaard, 1954. Da. Kr. 15.

These works, bearing the same title, were written for widely differing purposes.

Miss R.'s book is based on her James Bryce Lectures (1954), and adapted to the primarily Roman interests of the Collection Latomus. This dualism in its conception leaves one with some dissatisfaction. For an introduction to the subject too few examples are discussed in detail, yet for a more specialised study it retains too many of the generalisations and lists suitable to the original lantern lecture. Of the detailed discussions particular mention may be made of those of the Ostia Themistokles, and of the bearded head of the Bonn, Wilton House, and Louvre double herms. Miss R. wishes to revive the identification of the latter as Aristophanes. In her introduction she reminds us that in Greek portraits head and body must be considered together; a statement often made, but rarely illustrated so consistently and valuably as Miss R. does.

P.'s aim is different. He presents primarily a catalogue of the sixty-one portrait sculptures in the Glyptothek Ny Carlsberg, each illustrated and commented on with an account of its acquisition and a résumé of the work of scholars on each. This is preceded by a history of the collection and the work of P.'s predecessors; and an essay on Greek portraiture. This combination of both general and detailed makes the work as useful a short handbook as present disagreements about the basis of this study would allow.

The two books well illustrate this basic disagreement, Miss R. looking for an early development of portraiture, P. for a late. Crucial are their differing attitudes towards the Ostia Themistokles. In 'Three Critical Periods' Miss R. preferred a date c. 460. Her present discussion replies to such criticisms of that view as Miss Toynbee's in this *Journal* (1953, p. 189). Her argument from style is persuasive; and she adds a discussion of literary references to: (i) the existence of portraits before 450, (ii) the ancient representations of Themistokles.

But surely (ii) does not meet Miss Toynbee's demand for a genuine portrait-type before 450? If the Themistokles is an individualised portrait, dating it to 460 would demand a far-reaching reassessment of our view of the development of Greek thought and literature, as well as of portrait sculpture, such as the evidence of one head dated on stylistic grounds seems too slight to justify. Miss R. accounts for the appearance of an individualised portrait in 460 by the contemporary tendency towards realism. But realism by itself can only express a conception of the individual already existing; not generate it.

As to (i): what value are we to attach to Pliny's remarks about Hipponax and Theodoros? 'Der Begriff des Bildnisses . . . ist uns seit Jahrhunderten so selbstverständlich, dass es uns ungemein schwer fällt, die geschichtliche Betrachtung von Anachronismen freizuhalten', were the warning words of Pfuhl, unfortunately not known to Pliny. Miss R. interprets Pliny literally; but this is inconsistent with her own opening remarks about the lack of individualism in sixth-century portraiture.

P. adheres to the fourth-century date for this head: and

corresponding with these two datings are offered two accounts of the development of true portraiture. Miss R., allowing its origin in the early fifth century, traces its development in spite of the idealising tendencies of the fifth to fourth centuries (the possibility of which might have merited further discussion): P. waits till the early third century for 'le portrait profondément étudié sur nature' (that of Aristotle), distinguishing the works of the fifth century as 'le portrait moral, la physiognomie correspondant à l'œuvre de leur vie . . . sans être alourdi par la mesquine connaissance de son véritable aspect'.

The discussion might be helped at this point by a nicer discrimination in the use of the term 'portrait', and its relationship to such terms as 'realism', 'idealism', 'individual'. In principle, remarks such as Miss R. includes in her introduction are usually sound as far as they go. But in practice she does not distinguish portraits of those long dead (fifth-century, Homer, Anakreon; fourth century, Periander, Bias); of contemporaries (fifth century, Perikles; fourth century, Plato, third to second century, philosophers); and of those lately dead (fourth century Socrates, tragedians; third century, Demosthenes). All are portraits, but their creators' intention demands consideration. A study of this, with particular reference to contemporary literary developments, might help to resolve the basic disagreement revealed by the Themistokles.

Miss R. shows three hitherto unpublished heads in the Smithsonian Institute—Homer, Sophokles, Demosthenes. P.'s arrangement of photographs in other than numerical order is an unfortunate inconvenience; and we regret the absence from his essay of references in his citations of ancient writers.

H. H. O. CHALK.

**Musée National du Louvre. Catalogue raisonné des figurines et reliefs en terre-cuite grecs, étrusques et romains. I, Époques préhellénique, géométrique, archaïque et classique.** Text and plates. By S. MOLLARD-BESQUES. Pp. xii + 184. 108 plates. Paris: Editions des Musées Nationaux, 1954. £8 16s.

The Louvre has a superb collection of terracottas, amounting to about 6000. This, the first instalment of a complete catalogue, covers the pre-hellenic, geometric, archaic, and classical periods; every piece is described in detail, and almost every one illustrated. The author is to be congratulated on her scholarship and on the commendable brevity with which she displays it.

In her introduction, she points out that there are three ways of arranging such a catalogue—by types, by fabrics, and by proveniences. The first she dismisses as being unsuited to a Museum catalogue, the second as making too many demands on the subjective judgement of the author. But the third method, which she adopts, is really open to as many objections as the second, since proveniences are of many degrees of accuracy, a fact which M-B. does not appear sufficiently to recognise. Those from official excavations are obviously completely trustworthy; those given by dealers may be, and often are, completely fictitious; whilst many pieces have either no known provenience or a very vague one, such as Greece, South Italy, or Etruria. Under this system, pieces obviously of the same fabric, such as the Melian reliefs, are dispersed throughout the entire work, and others on the strength of a dealer's provenience (and a donor's provenience may well be a dealer's at one remove) find themselves in very strange company (was G 104 really found in Melos?).

A history of the Collection, which ends the Introduction, is informative and will be particularly useful when it is necessary to assess the trustworthiness of a given provenience.

The Catalogue itself is divided into three parts, pre-hellenic, archaic, and classical, for which the initial letters A, B, and C are used respectively. Within these divisions the pieces are listed by provenience (as given in the Inventory). The description of each piece is full without being wordy, clear, and careful. We are given in the first paragraph a description of the piece, the provenience (if an exact one is stated), and the date. A second paragraph gives the chief dimension and a description of the clay and of the decoration in colour. A third gives the inventory-number and details of acquisition, and a bibliography. The last paragraph is devoted to necessary comment, and publication of comparable material. These concise details include just about everything which we may want to know, except that references to comparable published material might well have been fuller; and the systematic and consistent arrangement enables the reader to pick out what he particularly requires with the minimum of effort. Finally, there is a useful concordance of inventory numbers and catalogue numbers.

The illustrations vary enormously in quality; some, such as B 52, C 53, and C 105, are excellent, while others, such as



B 134, B 150, and B 557, are poor. The better pieces surely deserve more than one view.

The most important parts of this excellent collection are the archaic fragments from Megara, the Boeotian bell-like statuettes and the archaic genre-figures of the same fabric (ploughmen, warriors, cooks, musicians and the like), the superb Corinthian sphinx head from Thebes (B 131), and the Cretan Dedalic figures. The collection excavated in 1915 at Eleusis in Thrace and the pieces from Cyrenaica will also be extremely useful to students. Rhodes, surprisingly enough, is poorly represented, the only really important piece being the Dedalic double figure of two women sharing a garment, from Camirus.

A few minor criticisms. Occasionally several pieces are catalogued together under the same number, for example, A 4 and 5 (pl. I) and C 121 and 122 (pl. LXXV). This is bound to lead to confusion. The faience figures on pls. XX, XXVIII and XXIX are not terracottas; it is hard to see why they were included. An Etruscan piece (B 542) has slipped in by mistake. As for the dating, in most cases there is no doubt that it is correct (or, where the piece is virtually undatable, the best possible); but, for the benefit of the non-specialist, the reasons for such dating could with advantage have been given. Finally, why no moulds? There must surely be a number in this collection, and the logical thing would be to include them with the related terracottas. Let us hope they will find a place in some future volume of this Catalogue.

R. A. HIGGINS.

**Corpus Vasorum Antiquorum. Great Britain, fasc. 12, Reading fasc. 1: University of Reading.** By P. N. and A. D. URE. Pp. x + 61, with 40 plates and 3 text figures. London: Geoffrey Cumberlege, for the British Academy and the University of Reading, 1954. 55s.

The Reading University collection of Greek vases was started as a teaching collection in 1911, when it consisted of barely twenty pieces; within thirty years, by acquisitions and gifts, together with loans from the Reading City Museum, it had become equal to a volume of the *CVA*, and now some two hundred further additions await a second fascicule, which we hope Mrs. Ure will have the opportunity of producing. It can justly be said that the Reading collection is the personal creation of the authors of this catalogue. There is one outstanding piece (the Pontic amphora, pls. 36-7), but the strength of the collection lies in the wide range of less ambitious vases, especially the Corinthian, minor Attic b.f. vases, South Italian r.f., and above all the great variety of Boeotian archaic and classical wares. The Ures' publication sets a new standard. The photographs are clear and attractive. Every view has been thought out; and stamped interiors and bases which play a part in the vase-decoration have received full attention, so far as possible without detriment to the profile views of the vases. The text is exemplary. The Ures have explored almost all the by-ways of Greek ceramics, and they acknowledge in addition the assistance of leading scholars in particular fields. It is doubtful whether any fascicule of the *CVA* has been written with so intimate a knowledge of ordinary Greek vases through the centuries.

Pl. 3, 1-9: some of the Protocorinthian aryballoi are perhaps dated too early; simple piriforms were flourishing in the Early Corinthian period. Pl. 8, 5 probably belongs to the considerable body of inferior Late Geometric in the style of Athens 897 (cf. *BSA* XLII 146 n. 1). Pl. 21, 4-5: late rosette bowls; no. 5 at least belongs to a class which continued into the late sixth century. Pl. 23 (Naucratis chalices): the colour of the slip is worth remark in every case, since the milk-white slip of the mid-sixth-century chalices perhaps offers a useful criterion of a relatively late date. Pl. 39, 1-3: such cothons do not seem to occur in East Greek.

J. M. COOK.

**Attic Vase Paintings in the Museum of Fine Arts Boston.** By L. D. CASKEY and J. D. BEAZLEY. Part II, by J. D. Beazley. Pp. viii + 104. Plates XXXI-LXIV. Boston: Museum of Fine Arts, 1954 (London: Geoffrey Cumberlege, Oxford University Press). £8 8s.

Since 1931, when the first part of this publication appeared, we have looked forward to the rest. Here we have Part II, and Part III is promised soon. It is not a catalogue, where one's relentless guide takes one from object to object, telling one all the essential facts about each (one hopes) and nothing more. Here, rather, a scholar who is also a connoisseur picks the best pieces from his cabinet, turns them in the hand for us and enlarges on anything they suggest to him. Since this cabinet contains Athenian red-figure vases of the near-century between Euphronios and the Kraipale Painter, and the connoisseur is

Beazley, we see much beauty and learn a great deal. To begin with the only criticism: though the proportion of photographs to drawings is high, should there not be a photograph of every vase and picture discussed? Both photographs and drawings are of beautiful quality and well reproduced. On one or two of the supplementary plates are illustrated photographs of fragments in other collections belonging or possibly belonging to Boston cups discussed here, as well as several parallel pieces: a minute fragment from, it seems, a replica of the Tyszkiewicz crater (in this connection, p. 16, l. 2, for 'Aphrodite' read 'Eos'; p. 20, l. 30, for 'Antiochos' read 'Melinippos'); and some fragmentary pictures of the deaths of Pentheus and Orpheus, illustrating discussions raised by Euphronios's psykter and the Niobid Painter's hydria. Points of detail: hydria no. 68 (pp. 9 ff.): Beazley finds parallels for the shape of the vase and for the physical types of the figures in the work of the Berlin Painter; I should have thought there was also some degree of stylistic influence from that Master's earliest manner, which Beazley seems to deny. Gallatin Painter's hydria no. 69 (pp. 11 ff.): Theseus's companions in the shoulder-picture look as though they had strayed there out of cup-tondos; I have always felt an affinity between the London Sleep and Death cup and the Diogenes Painter's work, and wonder if there may not be a workshop connection between the Munich-Gallatin-Diogenes-Syleus Painter line and the 'coarser group' cup-painters of the turn of the century. Cups nos. 71-81 (pp. 21 ff.): splendid series of Eleusis Painter, Protopanaiton Group, Panaitios Painter, and Onesimos, but I find these divisions difficult; 79 bis: I should have guessed that the woman on the interior was most probably picking something with her right hand out of a box held in her left. Bell-crater no. 94 (pp. 45 ff.): beautiful new photographs, beautiful new appreciations—one cannot have too much of this vase. P. 48: 'there is no other Pan in which the god shows so clearly through the goat'; I have wondered if the splendid god-mask in Berlin, generally called a horned Dionysos or Acheloos, is not a transfigured Pan—it was found at Marathon and cannot have been made more than a decade or so after the battle. Skyphos no. 100 (pp. 61 ff.): Semele also returned from the underworld, and would suit the Dionysiac context here. Volute-crater no. 108 (pp. 77 ff.): must not the figure within the house have been seated (like Thetis on the François vase and many others), or if standing a child, for her head not to be concealed by the lintel? Of the fascinating discussions one may particularly mention that on the confusion between Kephelos and Tithonos and that on representations of Anacreon.

MARTIN ROBERTSON.

**Griechische Vasen der reifarchaischen Zeit.** By R. LULLIES. Pp. 33, with 96 plates. Photographs by M. Hirmer. Munich: Hirmer Verlag, 1953. DM. 24.

This is primarily a picture-book: beautiful photographs of some of the best from the wonderfully choice collection of vases in Munich. As such it is particularly valuable, since most of these are best known through the now dating medium of Reichold's drawings. In some of the more general views highlights have been erased, which is a pity, but the standard of the photographs is exceedingly high, and in the best details the quality of the drawing is marvellously conveyed. The field covered is the first half-century of red-figure, black-figure being represented only by one side of the Andokides painter's bilingual, a profile of Exekias's eye-cup (as a prologue to the red-figure cup series), and the lid of the Kleophrades painter's early amphora. Personally I prefer to label this period Late Archaic, keeping Ripe Archaic for the perfection of archaic art in roughly the middle quarters of the sixth century: the Moschophoros and the Peplos kore, Nearchos and Exekias. From the generation of the vase-painters Euphronios and Euthymides at least, and earlier in sculpture, there is a new spirit, the beginning of the end of the archaic style. The last vase illustrated here, indeed, the famous cylinder with Sappho and Alcaeus, belongs almost purely to the new, classical, age. The choice on the whole is good. The Euphronios cup and the two Euthymides amphorae are among the finest Greek vases we possess; so also, perhaps, the Kleophrades painter's pointed amphora; the Andokides painter's amphora is, as the detail of Athena especially shows, among this second-rate painter's most attractive works, and the hydria by Phintias, with its interesting inscriptions, occupies a similar position. The Berlin painter is less happily shown than the other great pot painters. The silen panathenaic is splendid, but I feel it was a pity to pass over the beautiful panathenaics with athletes (one side of the earlier has never been properly published) in favour of the rather conventional Gorgon and Perseus and the curiously heavy and affected combat stamnos. Heavy and affected, too, is the Pan painter's elaborate psykter. It is of interest for its subject and



for its position in the artist's work, but one would have liked to see beside it the hasty but vivid column-crater with Thracian women. Cup-painters are less well represented: no Epiktetos, Panaitios painter, Onesimos, Makron; but the Brygos and one Douris interior (Herakles served by Athena) are lovely, and there are other good things, notably the Foundry painter's centauro-machy. The pictures are introduced by a short but reliable and scholarly text. A very nice book.

MARTIN ROBERTSON.

**Corinth. Results of excavations conducted by the American School of Classical Studies at Athens. Vol. I, part IV. The South Stoa and its Roman Successors.** By O. BRONEER. Pp. xix + 167, with 54 plates, 22 plans and 67 text figures. Princeton: American School of Classical Studies at Athens, 1954. \$15.

This volume is complementary to R. Scranton's on the Lower Agora, but unlike its predecessor it concentrates on a single great building. It rounds off the publication of the chief buildings of the Corinthian agora and at the same time makes a remarkable contribution to the history of Greek civic architecture.

Broneer first deals very briefly with the pre-stoa remains, which, he says, require further investigation before they can be fully published in a general account of early remains on the site. They include what is apparently a small underground shrine and a curious building which, according to Professor Charles Morgan, may well have been 'a tavern and house of entertainment in association with the cult of Aphrodite', in fact a modest forerunner of the shops of the stoa itself. The early buildings had quite a different orientation from the stoa, which cut ruthlessly through them and diagonally across the line of the ancient streets.

The full and lucid descriptions of the remains, the photos, and the drawings and plans by L. Holland, G. V. Peschke, and Piet de Jong, leave nothing to be desired. Broneer's reconstruction of the great building is convincing. The ground plan, with its shops containing wells for wine-cooling, and store-rooms, largely replaced in Roman times by various public halls and offices and an interesting fountain-house, is clear enough. The upper storey, in which a series of rooms with ante-chambers open off a corridor running along the back of the building, is more surprising. The evidence is slight, but it is sufficient.

The history and the use of the stoa are naturally more hypothetical. Broneer gives a cautious and very reasonable account. All the main criteria now seem to point to the latter part of the fourth century as the time of construction; but it is disturbing that a special piece of evidence, still not very satisfactorily accounted for, was thought for a time to date the building a century later. Broneer suggests that this grandiose design was undertaken at the time of the founding of the Corinthian League in 337 B.C. That the lower rooms were mainly taverns, though some may have been shops of other kinds, is clear from their contents; Broneer thinks that the upper range may have served as a kind of Grand Hotel.

The stoa is remarkable not only for its impressive and ingenious design but also for its fine workmanship and decoration. Various 'refinements' were introduced; the horizontal curvature was carefully carried out right down to the bottom of the foundation trenches. Broneer draws attention to the interesting series of mason's marks which have been detected, and to the need for further study of such marks.

The site provides striking evidence of the desolation of Corinth between 146 and 44 B.C. Yet the main structure of the stoa seems to have survived intact, so that the building could soon resume its functions and maintain its dominance over central Corinth. The upper storey was remodelled and the roof rebuilt. Broneer tentatively dates and assigns functions to the various structures which gradually replaced the shops and taverns—the only evidence for their identification is their form and decoration—and shows that this development, beginning at the east end, was piecemeal and followed no particular order or plan. One gets the impression that to the last the new elements were intrusions on the old coherent design, not fully assimilated. Several shops at the west end remained shops throughout, and indeed long outlived the newer public buildings. At Corinth there was never a thorough-going separation of the civic centre and the shopping district.

This fine volume shows again how much better we can appreciate the character of Greek civic architecture as a result of recent archaeological work. The existence of such a vast building, dominating the life of central Corinth for many centuries, would never even have been suspected otherwise. Pausanias passed by it—indeed probably through it—in silence, and went on to seek out the shrines of the gods and the more

modest structures which were already venerable antiquities in his day. But some of the old shops were still standing—perhaps he first paused for refreshment.

R. E. WYCHERLEY.

**The Athenian Agora. A guide to the excavations.** Athens: American School of Classical Studies, 1954. Pp. 110, with 8 plates, 1 plan and 19 text figures. \$1.

This is a highly capable collection of the less controversial facts about the Agora. Beginning with some of the more informative ancient texts on life there, it continues with a brief history of the area before describing each building in turn, and concludes with three appendices, the indispensable transcript of the route of Pausanias and notes on the water supply and on the remains of the Bronze Age found during the excavations. A date-table and a list of the monuments take the place of an index.

Most of the information is unexceptionable and conveniently arranged, although one must suppose that the reader's enthusiasm for Greece has been kindled elsewhere. One will search in vain in this book for any inkling of Attic elegance; and it will thus do nothing to dispel the contemptible but very fashionable charge of Philistinism brought against the excavators of the Agora by Osbert Lancaster and our more highbrow Sunday papers.

The work keeps clear, adroitly enough, of the numerous points of controversy still remaining. This perhaps improves it as an easy guide, but impairs its authority and attractiveness. Seldom, however, are its silences so unfortunate as on the Metroon. On pp. 39–40 the authors argue in effect that, according to the evidence of the excavations, no Metroon existed between 480 B.C. and the mid-second century, and that the archives were housed in the 'New Bouleuterion' from its completion in about 400 B.C. However, 'the name and function of the Metroon suggest that worship of the Mother continued in that area', *sc.* in the centuries after 480 B.C., apparently in the repaired 'Old Bouleuterion'. Not a word about the cult-statue of the Mother, variously attributed to Agorakritos and Pheidias. Normally, of course, a new cult-statue by such an artist presupposes the existence of a proper cella, an argument which these writers are very ready to adduce when it fits their bill. When, for instance, they have surmised on p. 48 (how safe is their argument?) that the style of the walls and foundations of the Temple of Apollo Patroos give it a date 'about 330 B.C.', they clinch their conclusion by the authorship of its cult-statue, as given in Pausanias.

Which versions of 'Solon's Laws' does our guide think were preserved in the Stoa Basileios? On p. 12 it cites Ath. Pol. 7, which says that the *Kurbeis* were set up in this stoa in Solon's own time. But did the stoa then exist? If the fifth-century Stoa of Zeus, found by the Americans, is the same building as the Stoa Basileios, as is very possible (see p. 51), the archaeological evidence shows that the Stoa Basileios had no forerunner on the site. On p. 52 (with no cross-reference to p. 12) we have the ambiguous sentence: 'Many public documents were put up in this building (*sc.* the Stoa of Zeus); among these were undoubtedly the famous law codes of Solon and Draco which were revised at the close of the fifth century before Christ.' Could one have been quite sure, from this guide alone, that even the revised codes stood here? And did the unrevised codes stand here before and/or after the last decade of the fifth century? Does the Ath. Pol. grossly pre-date the appearance here of 'Solon's Laws'? Was Ephialtes perhaps the first to bring down the *Axones* (identical or not with the *Kurbeis*?) to the Agora? If so, what *Axones* did Plutarch and Pausanias see in the Old Prytaneum on the North Slope?

To many, including evidently the writers of this guide, such uncertainties may seem irritating. Yet, if attractively presented, they would bring home even to the less-well-informed visitor some of the thrills and *επισημότητα* possible in Greek studies. Whereas cut-and-dried paragraphs, besides being foreign to the spirit of Ancient History, will only stupefy the casual and irritate the inquisitive.

One must not expect great elegance of orthography or printing in a small guidebook. But in no book does one like to see the word 'sacred' so hyphenated that its first syllable comes at the bottom of p. 31, its second at the top of p. 34.

As a guide for use on the site, this book has its defects. A large octavo, it is a little too big for convenience. With its thin cardboard cover, it will dog-ear and get rather dirty in the Athenian heat. The format is rather dreary, and the plates, largely reproductions of perspectives in water-colour wash (none, alas! by V. Peschke), fail to give the nuances of the original pictures and remind one too often of St. Ephraem Syrus, who, with skin the colour of parchment, had clothes the colour of mud.



The pictures of the Stoa of Attalos do nothing to mitigate this reviewer's regret at its projected restoration. To its other hateful features, all only too well attested, must now be added its ridge-pole and the segmental arch at either end of its interior. It represents, on the whole, the nadir of Ancient Architecture. The Romans had not yet taught the Greeks how to combine the Orders with confidence on Exteriors, thereby recovering the sureness of proportion and appropriateness of detail for which Classical work is generally so remarkable. It is a thousand pities that the average visitor to Athens will now derive his notions of a completed Classical building from an example of such singular depravity.

HUGH PLOMMER.

Ὁ στερεοβάτης τοῦ Παρθενῶνος. By KONSTANTINOS TH. SYRIOPOULOS. (Βιβλιοθήκη τῆς ἐν Ἀθήναις Ἀρχαιο-λογικῆς Ἑταιρείας, 32). Pp. vi + 131. With 9 text figures. Athens, 1951. Price not stated.

Four main theories have been advanced in attempts to date the stereobate of the Parthenon, generally explained as the foundation of an earlier temple: that it was built: (a) before 480 B.C.—Leake; (b) immediately after the Persian Wars—Dörpfeld; (c) before 480 B.C., but in two periods—(i) Dörpfeld, (ii) Heberdey; (d) after the Persian Wars but in one or two periods—(i) Kolbe, (ii) Tschira.

Mr. Syriopoulos, denying the existence of a 'Pro-Parthenon'—see Dinsmoor, *The Architecture of Ancient Greece*, pp. 149 f.—sets out to prove that the poros stereobate (στερεοβάτης and θεμελίον are throughout interchangeable terms) and marble temple belong together, both being Pericleian. His study is in three parts: (i) (pp. 1-6 and 34-5), a brief chronological survey of previous research—Kolbe's theory, *JDAI* 1936, pp. 1 ff., receives special attention; (ii) (pp. 7-34) a statement of the available evidence—derived from epigraphical and literary sources, excavation, and the present form of the building; (iii) (pp. 36-125), its examination and re-interpretation.

The building accounts of the Parthenon, printed in full, pp. 7-19 (= *IG* I<sup>2</sup>, 339-53), are claimed to include details of work on the poros substructure: S. would emend 339-41 to [Α]ἰδοῦντο τοῖς Ἀκτιστοῖς and λαβεῖν [αἰτίας] Ἀκτισθεν (pp. 47-8). A table (p. 39), based on an ingenious comparison of 'receipts' (λήμματα) and 'expenses' (ἀναλώματα) by years, seeks to establish the progress of work on the temple; this has an important bearing on subsequent arguments. S. attributes the apparent cessation of work in the third year (first suggested by Cavaignac) to the political opposition of Thucydides, son of Melesias. Historical arguments (pp. 40-5, and pp. 114-19) also favour the earlier date 450-49, here proposed for the commencement of the stereobate. The Parthenon and Theseion are thus brought into closer chronological relationship (cf. Koch, *Studien zum Theseustempel*, pp. 145 ff.). S. defines (p. 56) three building periods (a) 450-48, (b) 444/3-439/8, (c) 439/8-433/2 (the dates on p. 49 are discrepant), responsibility being assigned to Kallikrates for courses 1-20 of the stereobate (pp. 106 ff.), Iktinos, and Pheidias respectively. The building costs were chiefly defrayed by the Treasury of Athena and by the State.

The sections outlining the evidence afforded by excavation (pp. 19-34) and its interpretation (pp. 56-101) are prefaced by bibliographies which contain some repetition. Likewise the present arrangement of material renders repetition inevitable between parts II and III: total absence of indexes is a further complicating factor.

S., following Dörpfeld's plan (fig. 3, p. 24) and using his system of lettering (S<sub>1</sub>, P, S<sub>2</sub>, S<sub>3</sub>, K), describes the five walls South of the Parthenon (τοιχος and τειχος are respectively defined as temporary and permanent walls—p. 20, n. 2). Dinsmoor's division of the eight strata of filling material is retained, two broad classes of deposits being defined in relation to their position within or without the Polygonal wall S<sub>2</sub>. All walls except the poros angular S<sub>4</sub> are δρυχωματικοί. Attention is drawn (p. 26) to the common error regarding the relationship of S<sub>4</sub> and the Pelasgian wall (cf. Stevens, *Hesperia Suppl.* III (1940), p. 14). There is no synchronism between the stereobate and the South fillings and S<sub>2</sub>, so that chronological arguments from sherds discovered in those deposits are invalid. Moreover, limited traces of fire are not necessarily evidence of the Persian sack of the Acropolis, since the cause of the fire cannot be determined with any degree of certainty (pp. 100-1).

The stereobate itself and its courses are described in detail—Hill's measurements being followed (pp. 20-3 and pp. 87 ff.—p. 20, n. 1, lists, chronologically, excavations connected with the stereobate): its South side is illustrated (fig. 1, p. 21, and figs. 5-6, pp. 89-90). The method of its construction is suggested (pp. 87 ff.), namely that a cutting for the stereobate was made in the South fillings. The significance of the cutting discovered by Penrose in 1887 was not then realised (fig. 2, pp.

23 and 97). S. next considers its upper form (pp. 32-4 and 101-4) and the proposed restorations of a 'Pro-Parthenon' of 6 × 13, 6 × 14, 8 × 16, 8 × 17 columns. Hill's restoration (6 × 16), currently accepted, is examined in detail and held to be improbable (pp. 101 ff.). The titles Parthenon I, II, III, in reality reflect three plans for the same building.

Brief mention is made of the evidence for the existence of at least two pre-Persian sack temples (or treasuries) under the Parthenon (pp. 109 ff. and fig. 9). S. ends by summarising his general conclusions on the problems relevant to the stereobate together with his proposed alterations to current theories (pp. 120-5).

Misprints are few: the following corrections might, perhaps, be noted: p. 12 (345) should read 1-3 and 16-35: on p. 99, n. 2, for 407 read 417.

The nature of the subject has limited the author's choice of illustrative material to photographs from earlier publications. This does not, however, in any way detract from the value of this provocative study which merits serious consideration.

J. F. HEALY.

Studien zum Theseustempel in Athen (Abhandlungen der Sächsischen Akademie der Wissenschaften zu Leipzig, phil.-hist. Klasse, 47, 2). By H. KOCH. Pp. 158, with 57 plates and 140 figures. Berlin: Akademie Verlag, 1955. DM. 48.

Professor Koch's *Studies on the Theseion* form his modern 'edition' of the temple intended to replace that of Stuart and Revett (1794), the only complete architectural presentation.

The traditional identification of the temple as the Theseion has often been challenged since the nineteenth century. Professor Thompson, however, wrote in *Hesperia* XVIII, 230—perhaps prematurely—that the title Hephaisteion, first proposed by Sourmelis and Pervanoglou in 1868, was already generally accepted (cf. Gullini, *AG* 1949, pp. 11-38 and pl. III-X). The present author, therefore, who defines his position at the outset—'Für mein Teil habe ich einen jahrelangen stillen Kampf gegen die Hephaisteionhypothese geführt' (p. 11)—introduces his work with a not untimely reassessment of the evidence available for identification (Ch. I, pp. 9-15).

The Agora excavations have not resolved the problem, for, in the absence of inscribed dedicatory material, topographical arguments favouring the Hephaisteion attribution (based on Paus. I, XIV, 6: ὑπὲρ δὲ τὸν Κεραμεικὸν καὶ στοῶν τὴν καλουμένην Βασίλειον ναὸς ἐστὶν Ἡρακλείου cf. Dinsmoor, *The Architecture of Ancient Greece*, p. 180, n. 1) remain invalid, as K. implies, until the Stoa Basileios is identified with certainty: the topographical evidence adduced from Andoc. I, 40, by Thompson, is, in my opinion, similarly inconclusive. Again, the discovery of evidence of metal-working near the temple, a practice common to other sites (e.g. at Olympia and in Samos), does not prove the existence of a cult of Hephaistos. Lastly the temple sculptures represent the deeds, not of Hephaistos, but of Theseus and Herakles. Thompson's suggestion that Hephaistos may have been the subject of the West pediment, and Olsen's theory of indirect worship, are both dismissed (pp. 11-14). K. concludes his introduction by mentioning Theseus' connection with Phytalos (see also his article in *Studies presented to D. M. Robinson*, I, pp. 356 ff. and Ch. Picard, *Rév. Arch.* XXXIX, 108-10): he proposes for the temple the logical title 'Theseion-Herakleion'.

Nine important accounts of the Theseion, concluding with Dinsmoor's *Observations, Hesperia*, Suppl. V, 1941, are summarised (Ch. II, pp. 16-23): only that based on Stuart and Revett is treated in any detail. A bibliography of the numerous articles written since 1931 would usefully have supplemented Judeich, *Topographie*, pp. 365 ff.

A well-illustrated, detailed commentary on the post-Classical history of the temple, its site and preservation is given in Chs. III-V (pp. 24-41): it owes its remarkable state of preservation partly to the fact that it was converted into a Church of St. George by the Byzantine Greeks (pp. 35 ff.). There is an appendix on earthquake damage (pp. 41-3): to an appendix also might have been relegated the account (pp. 30-1) of the influence of its style on three later buildings.

A complete architectural survey of the Theseion, arranged in eight sections (Ch. VI, pp. 44-81, and pl. 40-57) together with a critical catalogue of the sculptures (Ch. VIII, pp. 109-44), forms the main body of the *Studies*. The building materials are discussed after the arrangement of Stevens, *Eretheum*, ch. 2 (pp. 44-5). K. agrees substantially with Dinsmoor's account of the construction of the foundation and steps (pp. 46 ff.), and with his plan of the marble floor slabs (p. 49). In describing the Cella K., disagreeing with Dinsmoor's sweeping conclusions, again infers, from the lead water-proofing of the walls, that the paintings planned for the Theseion, unlike those of the Pinakothek, were actually executed (p. 58). To p. 59, n. 3, should



be added the further reference *Hesperia* XIV, pp. 246 ff.—Broneer's article. Hill's arrangement of the inner colonnade (4 × 7 columns) is accepted (p. 60), and K. now agrees with Dinsmoor that the roof was of marble (p. 66) and not mainly of terracotta (contrast *Arch. Anz.* 1928, col. 714). The chapter ends with sections on Curvature (pp. 66–70), with conclusive measurements expressed diagrammatically (see further Koch, *Der griechisch-dorische Tempel*, pp. 34 ff.), and on Proportion (pp. 70–81), wherein Vitruvius' rules are experimentally applied to the Theseion (pp. 74 ff.), which is clearly not based on an 'inflexible modular system' (p. 80). Further relevant discussion of the architecture of the Theseion, doubtless inaccessible to K., occurs in the important article by H. Plommer, *BSA* XLV, 66–112, and pl. 7.

K. surveys treatments of the difficult question of polychromy in architecture (Ch. VII, pp. 82–104). Literary and epigraphical sources (pp. 87 ff.), and evidence from the Theseion itself are recorded (pp. 96 ff.), together with analyses by the late Professor Weygand for samples of blue and red pigments (set out as an appendix—pp. 104–8).

After listing older illustrations and plaster casts of the sculptures (pp. 113–17, and figs. 99–112), K. introduces their descriptive catalogue, commencing with the metopes (pp. 117–25, figs. 113–29, pls. 16–28). Of these No. 4 (East) is reconstructed with Eurystheus' head (p. 118), and K. follows Thompson, rather than Robert, in attributing No. 10 (East) to the Hesperides (p. 121). The East frieze (pp. 126–37, figs. 130–4, pls. 29–34), and the West (pp. 137–44, figs. 135–40, pls. 35–9) are described: the former is tentatively linked with the Pallantidai (p. 12), although certain interpretation is impossible. References to pedimental sculpture occur only incidentally in Ch. I.

In conclusion (Ch. IX, pp. 145–8), the main attempts at dating the temple are outlined: Tamajo, *Mem. Linc.* 1948, pp. 475–535, is omitted. K., for whom Parthenon and Theseion are as 'der Kathedrale und der Filiakirche', favours, with Dinsmoor, a date c. 450 B.C. for its construction.

Useful indexes occupy pp. 149–58. The several misprints occur mainly in quotation: these are not liable to mislead. The following corrections might, perhaps, be noted: p. 11, n. 3, for 'Hesperia 19, 1950', read 'Hesperia 18, 1949'; p. 36, l. 39 should read 'Dinsmoor'; and p. 63, n. 1, *AJA* 5, 1901: on p. 115, Nos. 22–4 need re-arranging: on p. 126 read 'Abb. 130–40'.

This important 'edition' of the best preserved Doric temple of the fifth century is admirably presented, and contains a wealth of excellent illustrations and drawings by the late E. von Stockar, for making which readily accessible Professor Koch also deserves our gratitude.

J. F. HEALY.

**La Forteresse de Rhamnonte (Étude de topographie et d'histoire).** (Bibliothèque des Ecoles françaises d'Athènes et de Rome, 179.) By J. POUILLIUX. Pp. 209, with 64 plates and 3 plans. Paris: De Boccard, 1954. Fr. 3,600.

This book was well worth writing; but the reader will discover this only when he reaches the last three chapters of the text and the large appendix, a valuable corpus of the Greek inscriptions from Rhamnonte. P. had hoped to excavate parts of the fort with Greek aid. But his plans fell through, and so he has collected what he can of its history from inscriptions and from the chaos left by Stais on the ground. The latter, as he admits, can tell him nothing certain. He is, besides, more practised as an epigraphist than as a topographer. He would therefore probably have done better to publish the inscriptions at the beginning of his book, follow them with his historical conclusions and relegate archaeology and topography to an appendix. By giving topography pride of place, he fails to do himself justice.

He gives two main reasons for the importance of the fort. It was on the safest route from Athens to Euboea, her 'grenier traditionnel' (was it not useful rather as pasture land?), and was also well sited for coasting vessels, beside the first truly calm water north of Sunium. Hence the curiously parallel histories of Rhamnonte and Sunium. He argues that the *enceinte* shows traces of at least two building-periods; an earlier, when the hilltop alone was fortified, with a weak wall; and a later, when more ground was taken in on all sides except the north-west, and enclosed in walls built for the most part of fairly good ashlar. The gate-wall on the south, of the later period, is especially well preserved. The fort, at its greatest extent, covered, according to P.'s plan, less than 40,000 square yards, one quarter the area of a normal legionary camp. Strangely, P. nowhere computes from this the maximum possible size of the garrison; and he tends to mislead his readers by calling the fort 'vaste'. Because the final walls make use of the 'indented trace', he considers

them Hellenistic. He notices also that the towers are solid. Comparable Attic forts, like Gyphokastro, tend to have hollow towers: and one might wonder, as P. does not, whether those of Rhamnonte were perhaps designed to support small machines of Hellenistic type. Literature and inscriptions seem to show that Rhamnonte was fortified during the Decelean War. When Euboea was lost to Macedon, it would become an important outpost, and, as we know from inscriptions, was garrisoned by Athenian *epheboi* after the army reforms of c. 334. In the earlier third century we find it held by mercenaries for Macedon, all under a mercenary *strategos*; but these include some Athenians, under an *epimeletes*. After 229 B.C. it is again under free Athenian rule. But this is its last period of prosperity. It was probably sacked by Philip V.

P. draws one or two interesting conclusions on isolated facts. For instance, his inscription No. 2 *bis* lists, he thinks, all forty-eight *epheboi* of the Erechtheid tribe serving for one year at Rhamnonte. Might one go further, and suggest a total garrison of about five hundred at this time (c. 330 B.C.)? He also appears to be the first to restore correctly the chief seats in the theatre, which served, as he well shows, as a general meeting-place of the demesmen and the garrison. For in demes like this the same enclosure was both theatre and agora.

One may criticise a few points in P.'s generally competent treatment of the inscriptions. The discussion and restoration of No. 11, a recorded decision of the 'soldiers' (= IG II<sup>2</sup> 1286), seems too speculative. No. 12 (= IG II<sup>2</sup> 2856) is evidently hard to photograph, but P.'s pl. XLVIII does not show how the stone ends on the right. No. 23 (= IG II<sup>2</sup> 2869) is not 'taillé en forme de chapiteau corinthien', but a sofa capital of the sort used on some Ionic antae. No. 35 (= SEG X, 210), recording the money owned by Nemesis in the mid-fifth century, shows that large sums were out, apparently on loan. But the totals involved, their constancy over the years, and the apparent absence of interest all show, despite P., that they were no ordinary commercial loans.

Some misprints remain unnoticed even on the large page of 'corrections'. On p. 28, line 4, one should surely read 'Sud-Est' for 'Sud-Ouest', and under pl. XXVIII, 2, one should read 'Aigosthènes, le rempart Est'. Elsewhere the topography is not always made clear. The general plan of the site is sparsely labelled, and one takes time to locate even the 'theatre'. The sanctuary of Aristomachos-Amphieraos (*sic*), is even harder to find, particularly because P.'s detailed plan of this includes no contour-lines. In order to study the general plan with the text, one has to remove it from the book. It should have been placed at the back. The map facing p. 14 could profitably have included a rather larger stretch of country.

P. appears rather narrow. Thus, on p. 50, he describes a wall with a rather wide rubble core as in 'la technique dite à εμλεκτον'. Presumably he borrows from RE. But perusal of Vitruvius II, 8, vii shows that there was much more to εμλεκτον than this. On p. 20 he discusses the stormy coast of S.W. Euboea, but shows no knowledge of Herodotus VIII, 7 and 13. He makes no comment on the genitive 'Αριστομάχου' in his first inscription, early though this seems (330 B.C.) for such a form. I wish he had made some concession to the general reader, who knows the genitive of Aristophanes only from the famous epigram.

HUGH PLOMMER.

**Samothrace. A Guide to the Excavations and Museum.** By K. LEHMANN. Pp. 101, with 51 text figures and a folding plan. New York: University Press, 1955. \$2.50.

The little island of Samothrace was best known in antiquity for its mysteries. Its more recent fame is due to the discovery of the Victory in 1863, which prompted the partial excavation of the sanctuary of the Great Gods by French and Austrian missions. This work was resumed by New York University in 1938, and has now been completed with a great access of knowledge of the earlier phases and fuller understanding of the main buildings; and a museum has been set up. This handy guide, which is prefaced by a synopsis of the island's history and a discussion of the cult, is written by the excavator and gives a foretaste of the final publication. The growth of the sanctuary can now be traced from its small beginnings in the archaic down through the splendour of the Hellenistic edifices and royal benefactions. Outstanding among the architectural results is the tentative reconstruction, with a date c. 340 B.C., of the little winged propylon (fig. 30), with elaborately necked Ionic columns, applied bronze frieze decoration of sacrificial paraphernalia, and the relief band of archaic dancing girls now located at eye level on the walls.

Fig. 42 looks like an Attic head-vase of the first half of the fifth century.

J. M. COOK.



**The Rhodian Peraea and Islands.** By P. M. FRASER and G. E. BEAN. Pp. viii + 191, with 12 plates and 2 maps. London: Geoffrey Cumberlege, 1954. 25s.

The *Oxford Classical and Philosophical Monographs* have been notably reinforced by this exemplary volume. It falls readily into three sections. The first publishes forty-three inscriptions, republishes six—these being the fruit of Bean's epigraphic journeys of 1948–50 to the Loryma peninsula and the country around Marmaris—with excursions to Mugla and the Gulf of Fethiye. In passing, we may note the outstanding interest of Nos. 8, 9, and 10: leases of approximately 200 A.C., with their curious sidelights upon Rhodian administration and husbandry. For the second section, devoted to the topography of the Rhodian Peraea, Bean again is chiefly responsible. Here, in the process of locating satisfactorily certain difficult townships—Amos, for example, to the north of Cape Marmaritsa, Tymnus at Bozburun—the important distinction is carefully maintained between the Incorporated and the Subject Peraea: between the projection of Rhodes on to the adjacent mainland and Rhodian conquests. In the last, and indeed the most substantial, portion of the work, where Fraser's expert craftsmanship is much in evidence, the Rhodian deme-system is carefully elaborated, with especial reference to the Incorporated Peraea. This is followed by an account of the administration of subject territory, more particularly in South-east Caria; and by a brief history of Rhodian expansion: the short-lived and troubled possession of Lycia, the early incorporation of the six neighbouring islands into Rhodian society, and, finally, the Nesiotic League, whereby Rhodes during the first half of the second century B.C. expressed her authority over the Cyclades.

I have described this as an exemplary book, and I should perhaps enlarge on this. That it is scholarly and meticulously accurate, from its authors' names goes without the saying, and it is not these qualities which I have primarily in mind. A pedestrian—a quite literally pedestrian—approach to history is proper to good epigraphy, for country must be walked over to study its inscriptions against the lie of the land and in relation to all surviving monuments. Professor Bean has walked his country, and that is ultimately the foundation on which the authors build their substantial structure—a structure which owes much to epigraphy and relatively little to the literary sources. And here I would add that the inscriptions throughout have been either discovered by the authors, revised by them, or, where no longer available, taken on the best authority: it is a matter for congratulation that we have at last no reference to outdated articles of the *Real Encyclopædie* or to the uncritical *Inscriptiones graecae ad res Romanas pertinentes*.

T. B. MITFORD.

# Les Constructions antiques de Délos. Documents.

By R. VALLOIS. Pp. xiv + 15, with 6 plans and 32 plates. Paris: De Boccard, 1953. Price not stated.

It is difficult to know what to make of this book. Perhaps the best a reviewer can do is to enumerate the things it contains, which, though good and useful in their way, are curiously limited. Pl. I is a general plan of the excavations ('état de 1919, partiellement complété en 1938'). Pls. II–IV give detailed plans of the remains in bits of the sanctuary—II, The Artemision and surroundings; III, the south-west part of the shrine, including the stoa and *oikos* of the Naxians; IV, the region of the Prytaneion (of the Temple of Apollo about one third is on III, two-thirds on IV). Pl. V is a plan of the north end of the Python ('Temple of the Bulls'), VI a restored section. Forty-two photographs follow. The first three show minor objects found in the north-east angle of the Temple of Artemis. The rest show buildings and monuments *in situ*, mainly from the areas shown in the plans, though there are also some pictures of the treasures, the agora, the theatre, and various oddments.

The Editor seems to have put together a rather haphazard collection from the material which happened to be available. The photos are mostly good, though some are very old (fig. 39—'Les fouilles du Théâtre, en septembre 1912') and in some, e.g. fig. 21, one gets a confused impression of the subject because the photo is not taken from a sufficiently commanding view-point. There is a catalogue of the monuments represented on pls. I–IV, classified and numbered, and a brief 'Explication des Figures'; and references are given to the author's *L'Architecture Hellénique et Hellénistique à Délos (A.H.D.)*. This admirable work, published in 1944, consists of three parts—I, the architectural development of the shrine of Apollo and the other shrines of Delos; II, types of building—temples, stoas, public buildings, gymnasia, fountains, houses, theatre, etc.; III, architectural details. It is thus a valuable work of reference both for the architecture of Delos and for the study of most types of Greek building. It is detailed and systematic but compact. By way of illustrations, however, it has only four small

figures in the text. In his preface, Vallois says, 'En dépit de mes soins, je n'ose me flatter que le texte soit toujours bien compris sans l'aide de figures. Aussitôt que possible, plans, dessins de détail et photographies paraîtront séparément sous le titre: *Les Constructions de Délos: Documents*. Ainsi puis-je espérer que l'indigence de l'illustration n'est que provisoire.' The present work is entitled *Les Constructions Antiques de Délos, Documents*, quite simply, without the addition of 'Vol. I' or 'Part I'. But it is very far from being a true companion volume to *A.H.D.*, and in fact is hardly complete and systematic even as part of a greater whole. One notes, however, in the preface a single casual reference to 'ce premier fascicule', and so perhaps one may hope for supplementation.

R. E. WYCHERLEY.

# Fouilles de Delphes. Tome II, Topographie et Architecture: La Colonne des Naxiens et le Portique des Athéniens. By P. AMANDRY. Pp. 128, with 41 plates. Paris: De Boccard, 1953. Price not stated.

'En attendant la publication définitive' is a familiar phrase to anyone interested in Delphi. In this case we have had to wait more than fifty years, but the excellence of M. Amandry's volume almost justifies the delay. Both these monuments were excavated at the time of the Grande Fouille; they have been discussed by generations of 'Delphiens' (and, of course, by the metic Pomtow). A. now combines their results with his own important conclusions from supplementary excavations and a re-examination of the remains to produce a truly definitive publication which leaves little for the reviewer to do but praise. The detailed descriptions are clear and accurate; the various problems discussed (and for the most part settled) with good sense and imagination; the illustrations, which happily include several of unfamiliar comparative material, superb.

The reconstruction of the Naxian dedication is certain—a cylindrical base, column of six drums (not seven as has been thought), Ionic capital and Sphinx, giving a total height of just over 12 metres and a ratio between height and diameter of column of almost 10:1. This, though still high, is nearer the norm than that given by earlier reconstructions. The hollows at the ends of the drums were explained by Pomtow and Homolle as designed for some kind of fastening, *empolia* or the like, and in spite of the structural difficulties which A. points out, I still prefer this to his suggestion that they were simply for the purpose of 'allègement'. The regularity of the cuttings seems designed for, and the proportions of the column demand, some kind of reinforcement.

A.'s discussion of the style (which includes some useful remarks on other Naxian works) leads him to a date in the decade 570–60 (the capital and its decoration slightly older than the Apollo temple at Naukratis and the Rhoikos temple and altar, the Sphinx a near contemporary of the Moschophoros and Kore 677). Few will disagree. About the purpose of the dedication (which Pomtow wildly associated with Lygdamis) A. wisely, refuses to guess. (The Sphinx of Naxos, on the other hand, which A. dates c. 540, may well be an offering of Lygdamis *post reditum* to a god whom he, like his friends Peisistratos and Polykrates, would find more sympathetic than the pro-Alkmeonid, pro-Spartan Apollo of Delphi.)

The Athenian Portico is altogether more complicated, and without fresh evidence, which is now hardly to be expected, a certain reconstruction beyond the columns and *krepis* is impossible. Of the roof, for example, nothing remains, and the plan of the east and west ends has been completely obscured by later alterations. But A.'s analysis of the possibilities is admirable, and his suggestions, both for the original design and subsequent modifications, take us as near an answer as we are likely to get. Particularly illuminating is his careful study of the distribution of the inscriptions on the polygonal wall, from which emerges, *inter alia*, an almost certain restoration of the interior with a series of wooden posts set regularly against the wall and designed to support the dedication, the *δῆλα* of the inscription.

And so we come to A.'s theory, already put forward in *BCH* 70(1946), about the date and purpose of the monument. For A. believes that these *δῆλα* were not arms but cables, the cables from Xerxes bridge across the Hellespont which the Greeks carried off in 479 *ὡς ἀνορθώσαντες ἐς τὰ ἱπλά* (Hdt. IX 121); that the Portico was built to commemorate the campaign of Mykale and Sestos. This ingenious suggestion is surely right. The style of the architecture and the letter-forms point to a date in the neighbourhood of the Persian Wars (it would be rash to be more precise) and therefore to a victory in the wars themselves, for although the inscription reads *τῶν πολέμων* and not, like the Marathon and Plataea dedications, *τῶν Μήδων*, the other Athenian successes of the period seem unlikely for various reasons given by A. and even more unlikely if we remember the political



situation at the time. I find it difficult to believe that anti-Ionian Apollo would be thanked for the Ionian victory of 497 or that the god who had opposed the original attack on Aegina would receive a dedication when Aegina was later defeated. It is true that Delphi had been unreliable against Persia, but after the national victory there was a definite policy of rehabilitation for the national sanctuary. The choice lies, then, between Salamis and the campaign of 479; and since the latter is not otherwise celebrated at Delphi, it is clearly to be preferred.

Thanks again to the inscriptions, A. gives a good account of the gradual collapse of the building from the first century B.C. onwards. Earlier remains he mentions briefly—we must wait for M. Lerat's long-expected volume for a description of the Mycenaean pottery etc. from the area.

W. G. FORREST.

**Fouilles de Delphes. Tome III, Épigraphie. Fasc. IV. Inscriptions de la Terrasse du Temple et de la Région Nord du Sanctuaire. Nos. 87 à 275.** By R. FLACELIÈRE. Pp. 179–299, plates 19–36, with 6 text figures. Paris: De Boccard (for École Française d'Athènes). 1954. Price not stated.

This fascicle of the epigraphical part of *FD*, edited by R. Flacelière, contains some very familiar pieces and some inedita of less importance. In accordance with the general practice of the series, commentaries are either lacking or are reduced to a minimum. The texts, revised by the editor and often confirmed by other epigraphists of the Delphian *koinon*, are for the most part well established, and are presented with the detailed accuracy one has learnt to expect from *FD*. Little comment is therefore required on their presentation.

The method of publication of the inscriptions in *FD*, according to the geographical position of monuments on which they are inscribed, is such that it is extremely difficult to know what inscriptions are contained in any given fascicle. Unfortunately, the habit seems to have won acceptance that temporary indices for the individual parts of each fascicle should not be provided. Thus the first part of the fourth fascicle of *FD* III, containing inscriptions 1–86, published by G. Colin in 1922 and 1930, contained an index, while this new part of the same fascicle, published a quarter of a century later, does not. Fasc. 3 of the series is in a worse plight: no temporary index or table of concordance (hardly less essential than an index) has yet appeared to the two parts of this fascicle, the first of which appeared in 1932, the second in 1943; in these two parts there are 441 inscriptions, all unindexed. In the present part of fasc. 4 there are another 178 inscriptions. It is true that *FD* is not the only culprit; *IG*<sup>2</sup> II/III and *Inscr. Delos* are equally unprovided for. At all events there can be no doubt that the absence of a temporary index seriously handicaps the general use of the work. It is difficult to see how we can expect those who are not specialists in epigraphy, and indeed (one might almost say) in Delphian epigraphy, but who have occasion to study a Delphian inscription, to find their quarry in this jungle.

For these reasons, then, the main task of a reviewer of this volume seems to be to indicate its contents, and especially to note those familiar inscriptions which reappear here.

The fascicle is indeed rich in notable texts. Nos. 87–119, inscribed on the pillar described as 'Monument d'un empereur romain (?)', are largely honorific inscriptions of the Roman imperial period, and the majority have been published, either half a century ago in E. Bourguet's *De rebus delphicis imperatoriae aetatis* (whence many subsequently appeared in *Syll.*<sup>3</sup>) or recently by F. himself in *BCH*, 1949. The inedita in this group (89–90, 92, 95, 106 and a few others) call for no special comment. On 94 (*Syll.*<sup>3</sup> 868B) F. does not refer to the important article of K. Praechter, *Hermes* LVII (1922), 481 ff., where the inscription is quoted in full (cf. also K. von Fritz, *RE*, s.v. Nikostratos (26), col. 547). 102 (unpublished) is an honorific decree for a citizen of Hadrianopolis: F. says 'La ville d'Hadrianopolis (qui devint Andrinople) fut fondée en Thrace par Hadrien en 132 etc.' Is it quite certain that this Hadrianopolis is in question? There are two decrees for doctors, both already known, 87 and 108; in his commentary on the former F. refers to other doctors honoured at Delphi. To the reference to 'BCH 52 (1928), p. 172–178 et 76 (1952) p. 651' should now be added *BCH* LXXVIII (1954), 68 ff., where L. Robert has republished, with an additional fragment, the second inscription published by him in *BCH*, 1928. To his list add the other inscriptions, *Syll.*<sup>3</sup> 437 and 538 (cf. A. G. Woodhead, *Camb. Hist. Journ.* X (1952), 242, T 56 and 58, who, however, makes no mention of either 87 or 108). 110, of c. A.D. 120, unpublished, is an honorific decree for a *φυσικός φιλόσοφος*. 111 (Bourguet, *op. cit.* p. 30) and 116 (unpublished) of the late first or early second century A.D., give the names of two *ἐπὶ τῶν ποιητῶν* of Tralles-Caesarea, in 111 the name is apparently Apollonius,

in 116 it is C. Pompeius Paullus. As F. points out the poet Hermesianax (*FD*, III, 1, 533–4) is also of Tralles-Caesarea. Another, considerably earlier poet, Theopompus of Megalepolis, is honoured in 145, of the end of the third century B.C.

132–35 are the inscriptions referring to the Attalid garrison at Lilaia in the first Macedonian war. These important texts had acquired a position rather akin to that still (alas) occupied by the *Res gestae divi Saporis*: discovered over fifty years ago (see Walek, *GGA*, 1913, pp. 184 ff.), many references had been made to them, but only one (134) had ever been published. Now, at last, we have them all, but, even so, there is still no commentary, though one is promised 'prochainement' from the pen of L. Robert, based on the dossier of M. Holleaux (cf. Robert, *Coll. Froehner* (1936), p. 96, note 2). It is a pity that, to console us in the interval, we could not be given a photograph. The four inscriptions are couched in the form of honorific decrees of the city of Lilaia for four different mercenary bodies and their leaders, described as ἀποσταλόντες ὑπὸ τοῦ βασιλέως Ἀττάλου ἐπὶ τῶν φυλακῶν τῆς πόλεως τῶν Λιλαίων. The honorific decree is in each instance followed by the names of those honoured, usually with their ethnic denomination. These lists, containing in all more than 230 names, provide rich material for prosopographical and allied studies, while the inscriptions as a whole are of the greatest importance for our knowledge of the organisation of the Pergamene army. It is to be hoped that the commentary of L. Robert will soon appear.

From the bases of the equestrian statues we have revisions of some familiar inscriptions, though most are more worn than when first studied, and the main difference between the new editions and previous ones lies in the underlining of certain letters no longer visible. 153 is a new edition of *OGIS* 228; 161 of *OGIS* 150: 163 of *OGIS* 234. In the commentary on 161 for 'Ptolémée IV Philométor' read 'Ptolémée VI Philométor', and for the most detailed discussion of Seleucus the son of Bithyia, with new evidence, see T. B. Mitford, 'Seleucus and Theodoros', in *Skript. utgiv. av Svensk. inst. i Athen*, 4<sup>o</sup>, 2, pp. 130–70 (cf. *JEAI* XL (1954), 138, no. 83; *REG* LXVII (1954), 180, no. 258). In 163, line 25, read not Ἀντίο[χου] but Ἀντίοχου, since Couve, from the copies of Homolle and Bourguet, read Ἀντίοχου.

The 'dédicaces éparses' contain a variety of dedications found in the area, including many rather insignificant inedita. An important contribution is made by the addition of a new fragment of the large Ptolemaic monument (*IG* IX, 2, 202), 233. This fragment, (c), clearly contains the (lost) name of a child of Euergetes I and Berenike, and this, as F. says, makes it more probable that the whole complex was erected in the reign of Euergetes, rather than that of Philopator.

The volume also offers a good deal to the student of archaic and classic epigraphy. 124 is a new edition of the Tyrrhene dedication, *Syll.*<sup>3</sup> 24; 147 of the Hermionian dedication, *Syll.*<sup>3</sup> 32; 176 is a republication of the Milesian dedication of the statues of the Hekatomnids, *Syll.*<sup>3</sup> 225 (Tod, *GHI*, 161B); 179 the Peparethian dedication, *Hist. Griech. Epigr.* 24; and, finally, 181–85 are the fragments of the Liparaean offerings.

P. M. FRASER.

**Die Astynomeninschrift von Pergamon (Abhandlungen der deutschen Akademie der Wissenschaften zu Berlin, Klasse für Sprachen, Literatur und Kunst, 1953, 6).** By G. KLAFFENBACH. Pp. 25, with 2 plates. Berlin: Akademie-Verlag, 1954. DM. 5.50.

A new critical edition of this celebrated inscription (*OGIS* 483, and *Add.*) is an event of some importance. The text has been widely discussed since it was first published by Kolbe in *Ath. Mitt.* XXVII (1902), 47 ff., no. 71, but all discussions have been based essentially on Kolbe's text. The original is still in Pergamon, and KL's text is based, not on a new collation of the stone, but on a squeeze in the archives of *Inscriptiones Graecae*, and the photograph provided by Kolbe (whence O. Kern, *Inscript. Graec.* p. 43), and reproduced again by KL as pl. 1. The text is, however, well-established, and KL's contribution, in this respect, is to suggest one deletion and the alteration of a few supplements, and to supply an apparatus criticus and a German translation, the latter a necessary adjunct to this difficult text. Apart from the text and translation there are two other sections: a discussion (pp. 11–19) of some difficult passages, and another (pp. 19–25) on the date and origin of the document. I take the second section first.

KL defends warmly (against J. H. Oliver) the traditional view, first expressed by Kolbe, that the inscription is of the early second century A.D. (Trajanic or Hadrianic), mainly on the obviously correct observation, already made by von Prott in 1902, that the hand is almost, if not quite, identical with that of the monuments of C. Antius A. Iulius Quadratus (*I. Perg.* 436 ff., *OGIS*, 484, cf. KL, p. 21 and pl. 2). Kolbe assigned the



original text to the regal period because the law is called τὸν βασιλικὸν νόμον, and because of the presence in line 205 of the month-name Πάνθειος, which, he claimed, would not be possible in an Imperial inscription (which, on account of the word βασιλικός, the law would necessarily be) after the introduction of the provincial calendar of Asia in 9 A.D. Kl. shows that the first of these contentions needs modification, and adds further arguments in favour of a date in the regal period; in particular, he notes the use of drachmae and not denarii to denote fines, and the presence of the magistrate called ὁ ἐπὶ τῆς πόλεως (line 69), who is not found in the Roman period. However, though he accepts and develops Kolbe's thesis (*op. cit.* p. 72) in regard to the original date of the law, he diverges from him in maintaining that it does not represent the introduction of the office of astynomoi. Kl.'s opposition to this consists, as did Kolbe's original assertion, in a bare statement. There is in fact nothing in the law to determine the issue one way or the other. The amount of detail in the text seems slightly to favour Kolbe's view.

In any case, the law is of the regal period. What, then, was the occasion of its re-inscription? The answer is held to lie in the fragmentary first line, which runs — ὁ ἀστυνόμος τὸν βασιλικὸν νόμον ἐξ ἰδίων ἀνέθηκεν. Kolbe, and all subsequent editors, assume that this line belongs to the occasion of the re-inscription, though, as we shall see, its interpretation is independent of whether it does or not. All previous editors, except Legras, have understood ἀστυνόμος as ἀστυνομήων, i.e. 'while x was ἀστυνόμος he exhibited [? see below] the royal law (sc. concerning astynomoi) at his own expense'. According to this interpretation, an astynomos of the second century A.D. paid for the exhibition (?) of the Attalid law out of his own pocket. Kl., following Legras, differs in his understanding of the Greek. He reads ἀστυνόμων and claims that τὸν βασιλικὸν νόμον by itself is not explicit ('es gab ja schliesslich mehr βασιλικοὶ νόμοι'), and needs qualification, and interprets thus: 'x dedicated the royal law concerning the astynomoi from his own resources'. He parallels the use of νόμος with the plain genitive, equivalent to ὁ περὶ τῶν ἀστυνόμων νόμος, and justifies its use here by the need for brevity in the heading. Further, he claims, 'das Fehlen des Artikels bei ἀστυνόμων erklärt sich aus dem gleichen Streben nach Kürze sowie der Voranstellung des Wortes'. He finally suggests (p. 25): [ὁ δὲ νόμος τοῦ δεινός ὑπὲρ (vel ἀντὶ) ἀστυνομίας] ἀστυνόμων τὸν βασιλικὸν νόμον, κ.τ.λ. 'd.h. also, dass jener so verdienstliche Unbekannte der hadrianischen Zeit das alte Astynomesgesetz aus der Königszeit als summa honoraria bei der Übernahme der Astynomie hat auszeichnen lassen'. I say nothing about the restoration, which is only tentative, but I feel that an interpretation of the surviving words which involves so many unusual features (the objective genitive after νόμος, the absence of the article, and the inverted word-order) can only be accepted as an ultimate necessity. Kl. (p. 24) speaks of the motive for the re-inscription as 'historisch-antiquarische Interesse', and something of this sort must, on the accepted interpretation, lie at the bottom of the re-inscription of an old law by a private citizen. But is it certain that the first line belongs to the time of re-inscription? ἀνέθηκεν, if understood (as generally: see, e.g. Kl.'s translation 'als Weihgabe aufgestellt', and again his note on line 1, where he refers to the inscription twice as a 'Weihung') as meaning 'dedicated', certainly favours this interpretation. But may the heading not be part of the original text? May not an official of the regal period have chosen himself to pay for the publication of the law regulating the activities of a body of which he was a member (supply, e.g. [ὁ δὲ νόμος τοῦ δεινός γραμματεὺς] ἀστυνόμων), perhaps because it was the inaugural law of the office? ἀνέθηκεν frequently means no more than 'set up', and this, surely, is a more natural interpretation here, where νόμον is the object of the verb. It is difficult to see how one 'dedicates' a law. A very good parallel to publication of a law by an individual with the use of the same verb, ἀνέθηκεν, though admittedly from the Imperial period, may be found in an inscription from Smyrna, recently published by J. Keil (*Wien. Anz.* 1953 (1), p. 17): . . . . . τῆς Μενάνδρου ὁ θεοφάντης ἀνέθηκεν, followed by a metrical sacral law. If, then, the verb does belong to the original heading of the inscription, which was repeated when the law was re-inscribed, the personal motive for the re-inscription disappears, and alternative explanations occur to one: for instance, the law may have been re-inscribed because it was still valid, some misfortune having overtaken the original text. That the original heading of the law should then have been preserved does not seem surprising. On the other hand, it is perhaps doubtful whether a royal law in the Hellenistic period would be called βασιλικός νόμος rather than, e.g., διάταγμα or πρόσταγμα. But we know too little about Pergamene chancery language to decide this point.

In his discussion of individual vexed passages Kl. always has something valuable and interesting to say, and shows a characteristic care and regard for niceties of language.

On line 17, a particularly vexed passage, Kl. maintains that in the phrase τὸ λοιπὸν διδόνον, λοιπὸν is 'schlechterdings unverständlich' and deletes it, rejecting the generally (but not universally) accepted explanation that the delinquent astynomoi pay the 50 per cent of the 150 per cent normally exacted from the defaulting citizen, while the latter pays only 100 per cent, gaining remission through the delinquency of the astynomoi. This seems illogical, and Kl. expresses himself strongly on the point: 'Warum in aller Welt, sollen die penitenten schuldigen Privatleute die zum Anderthalbfachen der Wiederherstellungskosten verurteilt sind (Z. 10f.) besser davonkommen, wenn die Astynomen ihre Pflicht nicht erfüllen, und in diesem Falle nur die einfachen Kosten bezahlen?' Kl. has logic on his side, but I am not sure that the argument is conclusive. For, even if the illogicality is an insuperable objection to the literal interpretation of the text as it stands, should we emend at this point a text which contains some notable obscurities and ambiguities in other places?

Improved supplements of the text are suggested by Kl. in other places, notably on lines 54 and 63. His notes on lines 95 and 97 strengthen and confirm the interpretation of Hitzig at this point, and his exact interpretation of στεγνά in line 207 is noteworthy.

This is a stimulating study of an important text, and is very welcome. [Since this review went to press the date of the inscription has been discussed again by J. H. Oliver, *Hesp.* xxiv (1954), pp. 88 ff. (cf. also J. and L. Robert, *Bull.* 1955, no. 188.) The text now reappears as SEG. xiii, 521.]

P. M. FRASER.

**Greek Coins.** By C. SELTMAN. Pp. xxvi + 311, with 64 plates, 9 text figures, and 4 maps. 2nd edition. London: Methuen, 1955. 50s.

During the twenty-odd years that have passed since the publication of its first edition, Dr. Seltman's book has remained the best arranged, most easily readable, and infinitely the best illustrated handbook on Greek coinage. As an introduction to this wide and most stimulating field of study it has been, already, of great service to student and scholar alike. The production of a second edition is, therefore, welcome, and the criticism that it might have been yet more valuable, if a greater attempt had been made to incorporate the fruits of recent research, is made only with due appreciation of the difficulties involved in the rewriting or revision of a work of this nature.

Seltman has, indeed, added a useful supplement to his select bibliography (pp. xxii-xxvi). He has also revised Chapters II-IV of his original work, dealing with the invention of coinage, the earliest silver coinage of Greece, and the first coins of Athens. Here there was certainly scope for revision. The trend of modern research has been to bring down the date of the earliest electrum coins of Asia Minor into the second half of the seventh century, and with them the beginnings of silver coinage in Greece to a period appreciably later than that previously accepted. This is a matter of supreme importance in the development of early coinage, involving not only the electrum series, but the dissociation of Pheidon from the first coinage of Aegina, and a date for the earliest Corinthian issues decidedly later than the period of Cypselus. In respect of the electrum Seltman has modified his dating of the first coins, but only from 700 to the first years of Gyges' reign (685-652 B.C.). His assertion that the vital coins from the Artemision by Ephesus are 'Kimmererschutt' and, so, pre-652 B.C., is extremely hazardous, as it seems impossible to exclude the possibility that four of them belong to the time of Alyattes, whose reign began only in 615 B.C. Many will regard the conclusions of E. S. G. Robinson's reconsideration of the Artemision coins (*JHS* LXXI (1951), 156 ff.) as too summarily dismissed; and the same may be said of W. L. Brown's article on Pheidon and Aegina (*Numismatic Chronicle* VI. 10.1950, 177 ff.). Nor will everyone regard the vase of c. 650 illustrated on p. 39 as conclusive evidence for assigning the first 'colts' to the time of Cypselus. In questions of such moment and in a book likely to be widely read by students, a broader, less categorical approach would have been welcome.

On the earlier Athenian coins Seltman has had some second thoughts, and has rightly relegated Peisistratus' mid-sixth century Pangaeon issues to the category of 'uncertain' (p. 50). But in this region he might have gone further and modified his sixth-century 'Paeonian' standard (p. 65) in accordance with the Thracio-Macedonian monetary system reconstructed by Mrs. Doris Raymond ('Macedonian Regal Currency to 413', *Numismatic Notes and Monographs* 126, New York, 1953).

Highly important to our understanding of Athenian monetary policy during the fifth century is the new dating of the Currency Decree, forbidding the use by Athens' allies of silver



coin, weights, and measures other than Attic, to c. 449 B.C. (cf. pp. 111 and 153). This clearly requires greater revision of the relevant sections, and in particular of the table on p. 151, than has been accorded it. The effects of the decree, with the emphasis on an interruption of coinage in the forties (and not after 415), has been admirably set out by Robinson in *Hesperia* (Suppl. VIII (1949), 324 ff.).

Of other points which might be made space permits only to remark that the rather detailed criticism in the review of Seltman's first edition (*JHS* LIII (1933), 128 ff.), regarding the difficult question of Syracusan fourth-century coinage, has been left unanswered. This, perhaps, merited rather more than the simple reiteration of the original argument.

J. M. F. MAY.

**Le sujet de la frise du cratère de Vix.** By J. DELEPIERRE. Pp. 31. Paris: De Boccard, 1954. Fr. 300.

The krater of Vix will long offer problems to the curious. The conclusions to which this pamphlet is directed are that the figures on the neck represent the Seven against Thebes, that the statuette on the lid is Hera, that the style and alphabet are Argive, that the krater was made at Argos, and that it was intended for some sanctuary of Hera or Diomedes at or near Spina. Mr. Delepierre's erudition is wide, though not always critical. The value of his work may be judged according to the reader's standards by the comment on Spina's dedications at Delphi (p. 29)—'l'importante colonie grecque de l'Italie septentrionale tournait ainsi sa piété—peut-être aussi ses espoirs de sauvegarde—vers le grand dieu qui, à Argos, avec l'épithète Λούκιος, était honoré du plus beau temple de la cité'.

R. M. COOK.

**The Stevensweert Kantharos.** By L. H. M. BROM. Pp. 29, with 9 plates and 5 text figures. The Hague: M. Nijhoff, 1952. 15 guilders.

In 1942 a silver vessel, measuring 105 mm. high and 127 mm. at its greatest diameter, and weighing 442 grammes, came to light in the bed of the River Meuse, near the village of Stevensweert in Holland. It passed into the hands of a Limburg collector, who seems to have been wholly unaware of its value and interest. It was not until 1949 that the piece came to the notice of the author of this brochure, a silversmith of Utrecht, who, realising that it is entirely of silver and of no small antiquarian importance, bought it and is still, apparently, its owner. In 1950 it was examined by Professor C. W. Vollgraff and pronounced by him to be a genuine classical antique. A short illustrated account of it by Mrs. Anne Vollgraff-Roes appeared in the *Illustrated London News* for 13 October 1951; and a long, learned, and fully illustrated paper on it was published by Professor and Mrs. Vollgraff jointly in *Monuments Piot* XLVI (1952), 39-67. The interpretations and theories offered in Mr. Brom's brochure seem to be based, in the main, on the Vollgraffs' researches.

The reviewer, having had no access as yet to the original, has not been able to check the accuracy of the account given here of the vessel's production. It is said to have been cast by the *cire-perdue* technique and to consist of three parts, each cast separately: (1) an inner cup with an elaborate double rim-border of alternating rosettes and palmettes below and leaf-moulding above; (2) an outer cup (round the upper edge of which the rim-border of (1) fits) heavily decorated with high reliefs, which were cast in a mould and chased after casting; (3) a foot cast solid and decorated with a Lesbian cyma border. (1) and (2) were formerly joined by soldering; and (3) was riveted and soldered on to (2). It seems that most of the decoration had once been lavishly gilded.

The decoration of the outer cup is arranged in two superimposed zones. The upper and broader zone, which is concave, contains six heads alternating with 'attributes'; the lower and narrower zone, which is convex, is filled with four exquisitely naturalistic sprays of alternating vine and ivy. In its shape and in the general scheme of its ornamentation, the Stevensweert vase immediately recalls the well-known pair of 'Maskenbecher' in the Hildesheim Treasure (E. Pernice and F. Winter, *Der Hildesheimer Silberfund*, 1901, pl. 13-16), which display a broad concave zone of masks, heads, and Bacchic emblems above, and a narrower convex zone of floral motives and birds below, but have plain rims and feet which, while similar to those of the new vase, are differently worked in detail.

The Stevensweert vessel is described as a 'kantharos'; and although it was found without handles, it is reconstructed with two in a drawing on p. 22. Each of the lost handles would appear to have consisted of two intertwined tendrils (cf. Pernice and Winter, *op. cit.*, pl. 17 (uppermost figure)), which sprang from the main stem of each of the vine and ivy sprays in the

lower zone on the body of the vase, that is to say, at four points at which pl. 7 and 8 of the brochure show clear signs of breaks. The addition of such handles certainly improves the vessel's appearance, as is also the case with the Hildesheim 'Maskenbecher' (cf. Pernice and Winter, *op. cit.*, p. 38, fig. 14), which Pernice and Winter think very probably had handles, although there is now no trace of where such handles had once been soldered on.

A drawing, showing at one glance the order of the heads and 'attributes', would have greatly assisted the study of the upper zone of ornament. Starting with a head of Pan, facing right (pl. 3 and 6), and moving towards the right, we see a fir-spray; a head, plausibly identified as that of a bearded Dionysus wearing a knotted *mitra* (pl. 2); a pair of cymbals; a head, found loose with the vase but subsequently lost, which was clearly that of Heracles, hooded in a lion-skin and facing to the left (*Monuments Piot* XLVI (1952), pl. 5 and 6 (lower figures)); a bow-case, quiver, and club; a youthful male head wearing a crown and ivy-wreath, with his mouth open and facing to right (pl. 5); a *thyrsus* and a *tympanum*; a head facing to left, which is lost apart from the locks of what seems to be female hair flying out behind (*Monuments Piot* XLVI (1952), pl. 5 (upper figure), p. 53, fig. 10); a *pedum* with fillet; a second youthful male head, similarly crowned and wreathed, but with closed mouth and facing to the left (pl. 4); and, finally, a *thyrsus* and a lyre. The whole *milieu*, including all the 'attributes', is obviously Dionysiac, with Heracles featuring as Dionysus' rival in the drinking-contest; and the second trio of heads must represent a Maenad flanked by two Satyrs. The Brom-Roes-Vollgraff theory that the female head is that of Cybele (unveiled and with tousled elf-locks!), flanked by the Dioscuri (ivy-crowned and without their characteristic pointed caps), is not likely to prove acceptable. *Pedum*, *tympanum*, *thyrsus*, cymbals, lyre, and fir-spray are all at home in the Bacchic revel-rout.

The most thorny problem raised by the editors of the kantharos is that of its date. The chronology of late-Hellenistic and early-Roman silver-ware is a notoriously subjective and controversial question, in view of our lack of pieces that can be securely dated by objective, external evidence. We can, indeed, infer from the so-called 'Megarian' and 'Homeric' bowls, which are obviously imitations of metallic prototypes in clay, that the hemispherical, footless bowl was the most characteristic silver vessel of Hellenistic times (e.g. C. Robert, 'Homerische Becher' (50. *Winckelmannsprogramm*), 1890; F. Courby, *Les vases grecs à reliefs*, 1922; M. Rostovtzeff, *Social and Economic History of the Hellenistic World*, 1941, pl. 25, 26, 47 (4), 60, 61 (3); R. J. Charleston, *Roman Pottery*, 1955, figs. 2 and 3). Some of these bowls show small-scale figure-scenes in low relief from epic or drama, and can be seen from the lettering of their inscriptions to range in date from the third century B.C. to the first century A.D.; while other bowls carry purely decorative motifs, of which the dominant one is a series of long, pointed, stylised leaves radiating from the centre of the base. But the vast bulk of extant silver pieces of this period came to light within, or on the confines of, the Roman Empire—at Pompeii, Herculaneum, Berthouville-Bernay, Alesia, Hildesheim, Hobby, Welwyn—in late-republican and early-imperial contexts. The Alesia *skyphos*, with its lovely naturalistic olive-sprays, must have been made before 52 B.C., since it was found in one of Caesar's siege-trenches (*Monuments Piot* IX (1902), pl. 16). But we have no proof that any of these vessels was made before the first century B.C., and some were probably made in the subsequent century.

We know from Cicero's Fourth Verrine Oration (21 (46-7)) that the use of decorated silver-ware, which was already ancient, was widespread in Sicily in the seventies of the first century B.C.; and some of the decorated vessels found on the above-mentioned sites could have been old when buried. With this in mind, we cannot absolutely exclude the possibility of the mid-to-late second-century B.C. date to which Brom and the Vollgraffs assign the Stevensweert kantharos. All we can say is that this kantharos fits most neatly into the first-century B.C. to first-century A.D. context. The closest parallels to its shape and figure-decoration are, as we have seen, from Hildesheim; the Lesbian cyma is closely matched on vessels from Welwyn (*Archaeologia* LXIII (1912)), Boscoreale (A. Héron de Villefosse in *Monuments Piot* V (1899)), and Hildesheim; and although regal Pergamon had developed, by the early second century B.C., a highly naturalistic floral style (T. Kraus, *Die Ranken der Ara Pacis*, 1953), such work seems to have been exceptional throughout the Hellenistic world until it enjoyed an extensive vogue in the Roman world of the late Republic and early Empire. Sprays of vine, ivy, plane, and olive, closely recalling the Stevensweert sprays, strewn over the sides of vessels are familiar items of decoration on silver pieces from Pompeii, Herculaneum, Hildesheim, and elsewhere (e.g. H. B. Walters, *Catalogue of Silver Plate in the British Museum*, 1921, pl. II (upper



figure)). As for the Greek inscription on the Stevensweert kantharos (see below)—it presents no evidence whatsoever that the vessel was dedicated in a temple of Zeus in second-century B.C. Sicily. Nor have we any reason to suppose that the M. Titinius, described as its owner in the Latin inscription, must be identical with the Roman officer of that name who suffered defeat in Sicily in 104 B.C.

J. M. C. TOYNBEE.

That the dating of this kantharos to the end of the second century B.C. finds support in the inscriptions on its base may be strongly disputed. Leaving aside the arbitrary ascription of the M. TITINI of the vase to the republican officer of that name (see above), we may observe that the Greek inscription—a series of letters, the forms of which are claimed, without argument, to be of the second century B.C.—is expanded into a hexameter, ingeniously indeed, but with a result that strains credibility. To do this at all, the initial A must be discarded and explained as a maker's mark; but, although there is a gap between it and the following Y or T, a similar gap separates the final C from its preceding K. Both letters, therefore, should belong to the main body of the inscription, or neither does.

Not all the letters are certainly what the editors wish them to be: those that they identify as Y, T, and Γ are very dubious. Why should supralineate Y appear first upright and then on its side, first without a tail and then with one? Not least, omicron must equal omega. All this Procrustean activity is in the interest of a restoration too hypothetical to win the confidence of even the most casual reader. Other restorations may be made with less (or more) plausibility: one may make of it a mark of ownership, a Christian dedication, or a curse, according to inclination.

But in fact the letter-forms cannot even be claimed to belong to the second century with any assurance. Note the variable use of apices, the rounded letters, the monograms, the abbreviations, the shapes of Z and Π. Of these features many may, indeed, be found in writing of the Hellenistic period, especially in the cursive script. But together in unison, as here, they suggest an attribution to the early imperial period, which coincides remarkably well with the artistic indications. To insist on dating them earlier is to press for the less natural interpretation in the face of the more natural one.

A. G. WOODHEAD.

**A History of the Athenian Constitution to the End of the Fifth Century B.C.** By C. HIGNETT. Pp. x + 420. Oxford: Clarendon Press, 1952. 35s.

The reviewer regrets that he is solely responsible for the delay in the review of this book, which is one of outstanding value to all students of ancient Athens.

The last few years have seen the publication of Jacoby's *Atthis* and the text and commentary of the *Atthidographers* in the same scholar's *F.G.H.*, *A Commentary on the Ancient Historians of Attica* (Nos. 323a–334), Vol. I (Text), Vol. II (Notes). Mr. Hignett was able to use the former in the preparation of his book, but not the commentary on the *Atthidographers*, though he was not aware, as he tells us in his preface, of Jacoby's views. His book, therefore, appears in timely fashion as a general re-examination of the confused and obscure and very frequently unsatisfactory sources, especially the *Athenaion Politeia*. In dealing with the latter in particular he takes the critical attitude of Beloch and de Sanctis, which is to the good, for those who feel more confidence in the sources of our information and their interpretation will know how to react, while those who are not specialists in Athenian constitutional history cannot but benefit from an encounter with Mr. Hignett if they are prepared to exercise their own judgement. Most will agree that he has done right to stop where he does, at the end of the fifth century, and to omit the 'minor modification of detail and adjustments to changing conditions' of the period to 322 B.C. It is not so easy to decide how much political history should be included: any account of the period 510–480, of Themistocles, Cimon, and the Decline and Fall of the Athenian Empire, must introduce a good deal of background. Some points can be treated in an appendix (because of their importance we cannot say 'banished' or 'relegated'), such as The Date of Solon's Legislation (Appendix III), Peisistratus and the Philaidai (V), The Date of Cleisthenes' Reforms (VI), Chronological Problems in the History of the Years 506–480 (X), The *Strategia* (XI), The Revolution of 411 (XII), The Installation of the Thirty (XIII). Most of these deal with problems and material of first-rate importance; there has been a tendency on occasions (witness Jacoby's *Atthis*) to relegate too much to appendices and 'footnotes' (with an inadequate index as in *Atthis*). Hignett does not fall into this error, and indeed attains

a happy mean in most cases between what he includes in the text and what in appendices.

The basic divisions of his treatment are the obvious and essential ones: the Sources; from Monarchy to Aristocracy; the Aristocratic State; Solon; from Solon to Cleisthenes; Cleisthenes; from Cleisthenes to Ephialtes; the Revolution of 462; Radical Democracy; Decline and Fall of the Athenian Empire (rightly introduced by a section headed 'The Supremacy of Pericles'). From time to time the sources are particularly scrutinised in the text (as in the case of the reforms of Cleisthenes), and the whole approach is critical. If it is right to point out that Hignett is at times as prone to conjecture as the best, it is only fair to add that the bases for his conclusions are nearly always fully presented. The book does not make easy reading (especially in some of the appendices), as might be expected, but at times abbreviation in exposition or argument leads to a certain obscurity which could have been avoided, though at the cost of increased length.

There are many good points: 12 ff. on the documentary evidence in general: throughout the earlier part of the book Hignett's great service is to make quite clear the degree of our knowledge or ignorance. He shows excellent sense on Peisistratus and the *Hyperakrioi*, on the authorities for Cleisthenes' reforms and on Cleisthenes and the Areopagus. He rightly rejects, and with good arguments, a Cleisthenic introduction of ostracism. There are many others, too numerous to detail here, throughout the book.

It is certain also that many details will provoke criticism and alternative interpretation. 47–8. *Demiourgoi* as 'craftsmen'. Palmer's suggestion that this group represented holders of 'public' land (Inaugural Lecture, 'Achaean and Indo-European' (Oxford, 1953), 12 ff.) makes an interesting comment on this idea. 53. On the Ionian four tribes, here bound up with the colonisation of Ionia, which seems to be dated to the twelfth century. More is needed here on this thorny problem. How does the Ionian four-tribe system fit into the gradual (?) process of the unification of Attica? 83. Could cavalry ever be a 'chief arm in war'? 84–5. Hignett hardly discusses sufficiently the development of the hoplite phalanx. The chapter on Solon gives the least satisfactory impression of any in the book, but this is inevitable. Hignett sensibly rejects (88) the introduction of coined money as the cause or main cause of the economic crisis. If the introduction of coinage in East Greece and in Aegina is dated later than is usually done (NC 1950, 189) his view is strengthened. Hignett is not very clear about the *Hektemoroi*: were they debtors who could not alienate their land because of family not individual land-tenure? Something might here have been said about the problem of land tenure and the question of the acquisition of land by those who had made fortunes in other ways (there must have been some of these even in non-commercial Athens). Hignett seems correct in stressing the political implications of Solon's reforms and of the crisis which produced them. But whom does he have in mind when he says (88), 'Solon's supporters included not only the farmers, but some of the rich and influential citizens as well'? It is not clearly explained by defining them as (102–3) landed non-Eupatrid farmers. Hignett's discussion of the supposed Solonian Council of Four Hundred is a valuable one (he makes a good point in connection with Aristion's proposal on behalf of Peisistratus). But if it can be argued (149–50) that the Council of Five Hundred was created by Cleisthenes as a probouleutic organ 'not encroaching on the authority of the Areopagus but providing a necessary safeguard against the abuse of power by the magistrates and the popular assembly' why need such functions be disputed for the Solonian Council? 95. On Sparta, the Chiot inscription (Tod 1) and acceptance of the Solonian council, see now A. Andrewes, Inaugural Lecture, 'Probouleus: Sparta's Contribution to the Technique of Government' (Oxford, 1954), pp. 21–2. I cannot follow Hignett's argument from the Ionians' greater (cultural) development which made 'them politically mature enough to experiment with constitutional novelties...'. 98. The question of the admission of the *thetes* to the Assembly is a thorny one. In 98 they seem to be called 'landless men', yet according to the classification of the *tele* they might enjoy up to 200 measures income per year. The status of the fourth class is one on which Hignett can clearly not make up his mind (see 101). 100. Meaning of term *Zeugites*? 117–18. The question of the *Thetes* comes up again. The *Thetes* are called 'landless men'; whence, then, came their income, and how was it equated with measures of corn or oil; and does this apply only to the *Thetes*? 140. The phratry position is made too easy by Hignett, as in 143, likewise, the question of money equivalence seems to be passed over too lightly. 153. The date of Themistocles' archonship is too readily put in the nineties (Cadoux, *JHS* LXVIII (1948), 116–17, dismisses Gomme's view too lightly), and the whole explanation of the rise and career of



Themistocles carries little conviction (159 ff.). 175. No satisfactory reason emerges for the rise in importance of the strategoi, especially as the importance of the archons has been stressed in the period 507-487 B.C. (153). 181-2. Miltiades' 'inflexible hostility to Persia'. Is this certain; did Darius know of it (Her. VI 41)? 182. The Alcmaeonidae pro-Hippias in 490 B.C. A surprising conclusion, and the possible origin of the discreditable story as an anti-Periclean slander deserves a mention. On the whole period 500-479 B.C. (166-92) Hignett displays something less than his usual caution and forgets the considerations set out by Gomme in an article 'Athenian Notes', *JHP* 1944, 321-39, which should figure in the Bibliography. 180. On the Elder Alcibiades see now *Hesperia* XXI (1952), 1-8, 226. Restriction of the *strategia* to holders of land. This needs more discussion as also does the interesting question of the effects of the decline in the value of money and rise in the price of commodities on the property classes. 255. The citizenship law is also fully discussed in an Appendix. But need we see in it more than an attempt to restrict the number of those enjoying the advantages of Empire? 256. The 'three parties'. Are these the divisions mentioned 260, with a too facile equation of rich, rural population and the urban proletariat with oligarchs, moderates, and radicals? But what of a man like Nicias? 257. On Pericles' 'pacific policy' cf. now Beaumont's suggestion on the Acarnanian alliance as belonging to 433 B.C. (*JHS* LXXII (1952), 62). 259. 'By 431 nearly half the Athenians lived in Athens itself or the Piraeus.' But did not many own property in the country and did not this influence their attitude to war with Sparta? 260 ff. The effects of the demagogues are well set out; their origins and economic and social status are barely discussed. The period after the Peace of Nicias with demagoguery on the one hand, and extreme party discipline on the other (267), with Alcibiades as a complicating element, presents a strange aspect. Hignett hardly does justice to it or to the narrow line between demagogue and oligarch as represented by Peisander, Phrynichus, and Critias.

But these are small criticisms to set against so much that is well thought-out and well expressed, provocative but not too much so.

R. J. HOPPER.

**Antigono Dosone re di Macedonia.** By M. T. PIRAINO. Palermo: Presso d'Accademia, 1954. Pp. 77. Price not stated.

This study of Doson is on the whole rather slight; it is a pity that Miss Piraino was not able to tackle the subject on a somewhat more ambitious scale. The reign involves many debatable problems, and it may be well to mention her attitude to some of them. She believes (surely wrongly) that Chryseis was the concubine rather than the wife of Demetrius II (p. 7); she is non-committal on the battle of Andros; she discusses neither the view of P. Treves (perhaps because she regards J. V. A. Fine's criticism as adequate) that Doson engineered the Cleomenean War, nor that of F. W. Walbank that Doson may have been inspired by Aratus in creating the Symmarchy, nor Treves' view that the Macedonian people remained outside the Symmarchy (she does, however, reject out of hand the hypothesis that the *κοινὸν* of *Syll.* 575 was an organ of the Macedonian state; p. 6, n. 19); she follows Treves in the view that the Symmarchy was primarily anti-Roman and believes that Doson's alliance with Demetrius of Pharos also shows his policy to have been potentially anti-Roman; she does not mention the doubts felt by many about the tradition of Rome's relations with the Acarnanians in 239. In regard to Doson's Carian expedition she has unfortunately failed to notice references, published before 1954, to the important Carian inscription which shows that Philip V had authority there in the third year of his reign (see L. Robert, in *Holleaux, Etudes*, IV (1952), 162; P. Fraser, *JHS* (1953), 170). Thus Doson's authority in Caria will have been maintained throughout his reign and he will not have handed Caria over to Ptolemy (as suggested on p. 43) as a *quid pro quo* for a free hand in Greece.

One basic problem of Doson's reign is the chronology. The date of the death of his predecessor Demetrius II is generally agreed to have been 230/29 B.C., but attempts at greater precision have not reached agreement. The majority of historians would place it early in 229; Beloch hesitated between winter 230/29 and some date in the summer or autumn 229. Now Miss Piraino rightly follows Holleaux in rejecting Beloch's attempt to place the Roman expedition to Illyria (which Polybius 2.44.2 roughly synchronises with Demetrius' death) in 228 instead of 229, but she places (p. 3) Demetrius' death as early as 'the beginning of the summer of 230' because she believes that Gonatas died 241/40 (p. 61). This should have been argued in greater detail (a mere reference to E. Mann's views is scarcely enough, since it is far from certain that they are cor-

rect); this should have been done because it affects the chronology of later events: thus she assigns Doson's Dardanian campaign to 230, the Thessalian to 229, and the Carian to 228 (this last is removed from the more usual 227 for no very cogent reason). It is unfortunate that she did not know the new inscription from Demetrias 'To king Antigonus and Philip' (*Bull. Corr. Hell.* 1950, p. 42), which is important for the consideration of Doson's position as *ἐπίτροπος* and *βασιλεύς*.

Some of Miss Piraino's compatriots have recently written on Macedonian affairs at very considerable length. She has commendably aimed at greater brevity of treatment, but it is a pity that she has not discussed many of the problems more fully and has not attempted to assess more closely the achievement of Doson.

H. H. SCULLARD.

**Pirro. Aspirazioni egemoniche ed equilibrio mediterraneo (Università di Torino, Pubblicazioni della Facoltà di Lettere e Filosofia, V, fasc. 2).** By G. NENCI. Pp. 199. Turin, 1953. Price not stated.

This work is not a full-scale 'Life of Pyrrhus', with detailed discussion of all his military and other activities, but rather, as indicated by the sub-title, an attempt to assess the significance of his career in the wider setting of the political relations of the Mediterranean world. Only against this background, Nenci rightly believes, can Pyrrhus' career be properly understood. But an analysis of Pyrrhus' world must be preceded by an evaluation of the view-point of the historians. Neglect of this, N. believes, has resulted in the *communis opinio* that Pyrrhus was primarily an adventurer, a view that has arisen because political evaluations have been based too exclusively upon biographical elements.

The views of R. Schubert (1877) and B. Niese (1896), who tried to differentiate between the pro-Pyrrhic Greek sources and the anti-Pyrrhic Roman sources, have not been substantially superseded, but for N. they are inadequate because they neglect the influence of Pyrrhus' own *ὑπομνήματα*, which will have been not a 'diary' but a retrospective official justification of events. N. attempts to trace this tradition (in, e.g., Frontinus and in Diodorus via Timaeus) and believes that the primary nucleus of our sources is the influence of Pyrrhus' personality, i.e., a biographical nucleus, with ethical interest. Rome's contribution was curious: instead of decrying Pyrrhus' personality, it sought to increase the greatness of his Roman opponents (Curius, Fabricius, Claudius) by contrasting their loyalty with his. Only three Greek sources escaped Roman influence: Lycophron (ll. 1446 ff. refer to Pyrrhus, not Alexander; dated c. 273 B.C.), the papyrus edited by W. Schubart (*Griech. Liter. Pap.* 1950, n. 39) referring to a (probably third-century) moralising work on Pyrrhus, and the passage from the Cynic Teles (Hense, p. 43), which derives probably from Bion and, as Tarn first showed, refers to Pyrrhus and Gonatas. N. believes that only Pyrrhus is meant, and he would trace to a Cynic source the *topos* in the report of Cineas (Plut. *Pyrr.* 19), who represents Cynic reason opposed to Pyrrhus' desire for action. Apart from this element, which represents a Cynic *Life*, critical of Pyrrhus' ambitions, the popular Hellenistic biography was laudatory, emphasising Pyrrhus the warrior, his severity and humanity, and above all Pyrrhus as the enemy of Rome. From this moralising and often *ex eventu* attitude to facts we must attempt to free ourselves by turning to the political, social, and economic problems of the Hellenistic world and the evidence afforded by indirect reference, archaeology, epigraphy, and numismatics.

All the interesting problems and speculations through which N. leads his readers cannot receive adequate notice here, still less discussion, but his main thesis must be sketched. He finds the keys to Pyrrhus' early policy in his use of genealogy to create national unity and to justify the need of a *novus homo* for nobility and *Grecità*; his divinity; his coinage for propaganda; his title of King. But behind his policy lay the shadow of Ptolemaic power: Alexandria above all aimed at establishing a peaceful world in which its trade could flourish, and it achieved this by 'harnessing Pyrrhus to the chariot of Lagid power' (p. 98). When Pyrrhus returned from Egypt to Epirus, married to Ptolemy's step-daughter, his destiny was already largely determined. N. argues that Ptolemaic foreign policy employed conquest only for commercial ends; that Ptolemy's adoption of the Phoenician coin standard in 305 and its exclusive use after 285 represents an anti-Carthaginian policy with an eye to the western markets; that thereafter this policy was consistently maintained; that earlier Egypt had supported Agathocles and indeed had inspired his African expedition; that after his failure it turned to Pyrrhus, whose philo-Ptolemaic policy made Epirus a real international power; and that, Pyrrhus having failed, Egypt finally turned to Rome in 273. This reconstruction of Ptolemaic policy will certainly not remain unchallenged, while



N.'s further thesis is equally provocative. With Ptolemaic economic expansion in the West blocked only by Carthage, and with Pyrrhus from 297 to 280 an agent in the wider field of Ptolemaic policy, Pyrrhus' expedition to the West must form part of Ptolemy's designs: it was in fact directed not against Rome but against the common enemy of Egypt and the Western Greeks—Carthage. For Nenci, all Pyrrhus' military and diplomatic activities in Italy and Sicily, and Rome's relations with Carthage, suggest this interpretation. Manifest objections will immediately occur, to many of which N. has offered anticipatory answers, but if Pyrrhus really aimed at Carthage alone, why did he ever involve himself in the Tarentine request for help in a war against Rome in Italy (even though he may have underestimated Roman power); why rather did he not indicate to the Syracusans that he would have welcomed in 280 that invitation for help against Carthage which they in fact sent two years later? (N. would say that he needed the Tarentine fleet.) May not his desire not to fight Rome date only from after his initial failure in Italy when his second (rather than his exclusive) aim may have appeared the more desirable?

Individual problems cannot be discussed here, but it should be emphasised that this book is an important contribution which will be welcomed by everyone interested in the early years of the third century. N. shows mastery of the ancient evidence and the modern literature. Original and thoughtful, he argues clearly and does not fear to challenge accepted opinions. He discusses many problems of the period in considerable detail, and much that he has to say is illuminating and convincing; and even where a reader may hesitate to follow, he will still find N. stimulating and persuasive.

H. H. SCULLARD.

**The Theory of the Mixed Constitution in Antiquity. A critical analysis of Polybius' political ideas.** By K. VON FRITZ. Pp. xiv + 490. New York: Columbia University Press, 1954. \$7.50.

This is a stimulating book, of wide learning and marked by exemplary lucidity. Von Fritz's main subject, best indicated in the sub-title, is Polybius' political thought. A study of his life, political and intellectual background (Stoic influence is not rated very high), and the principles and composition of his history forms the indispensable background; his political theory is then analysed and evaluated in the light of an elaborate re-interpretation (often very acute) of the development of Roman Republican institutions, and in comparison with Hobbes' view of sovereignty. I cannot here attempt a summary of his close reasoning; those who care for the study of Greek political thought, of Polybius and of Roman institutions must read the book. I shall confine myself to observations on a few points of concern to the Hellenist; dissent implies no disrespect to von Fritz, whose views command attention and provoke further thought and enquiry.

He rightly traces the idea of the mixed constitution to the belief that power corrupts combined with admiration for τὸ πλεον, voiced first in a political context by Solon (echoed, as he might have noted, in *Eumen.* 526 sqq.; 696). Fear of unrestrained power in itself might produce the doctrine that government must be subject to fixed laws; this could be, but did not need to be, linked with the theory of the mixed constitution (cf. *Politicus*). Von Fritz (p. 219) says that P. had at least grasped that at Rome there was no authority above the law. But P. is not expressly making that point; and did the discretionary character of *imperium*, exemplified in the growth of *ius honorarium*, and the sovereignty of *comitia* and presiding magistrate, when in agreement and not checked by veto, correspond to Greek notions of the reign of fixed laws?

Von Fritz finds no clear expression of the theory before Plato's *Laws*. But Plato was not its sole originator. About the same time Isocrates (vii 26-7; xii 130-2, 153; iii 24 is hardly relevant) and unnamed theorists to whom Aristotle alludes (*Pol.* 1265b 33; 1273b 39) were preaching other versions. We may take it further back. Aristotle knew of those who regarded Solon's constitution as mixed. The *Menexenus* 238c says that Athens has always been an aristocracy, though some call it a democracy and others by other names. Athens has *basileis*, yet the people controls most things, while the best men secure office. Now Aristotle (*Pol.* 1294b 34) suggests that there is no better mark of a mixed constitution than that it may be (in exactly) called either aristocracy or democracy; and the mention of the unimportant *basileis* shows that we have here an attempt to wrest the ideal of the mixed constitution to fit democratic Athens and to show that it is the *patrios politeia*, for which democrats as well as oligarchs could claim to stand. The allusive character of the attempt shows that the theory was already in the air, surely a product of the endemic *stasis* of the late fifth century. There is then no difficulty in finding an

earlier allusion in Thucydides' *μετρία ἐς τοὺς ὀλίγους καὶ τοὺς πολλοὺς γύγκρσις*; Mr. de Ste Croix shows (*Historia* V, 1 ff.) that the moderates of 411 did not (as von F. thinks) limit the franchise. The mixed constitution was thought (as in 411) to ensure safety against foreign enemies by establishing internal stability (cf. *Laws* 692b). But it was also a dodge to delude the masses, appropriate in 411 as later; checks and balances normally suit conservative interests, and the 'justice', at which it professedly aimed, was the distribution of rights, where, as the best people thought, they belonged; the people are only to think that they enjoy equality (cf. *Cic. de leg.* iii 24).

Von Fritz does not discuss all variations of the theory. His instructive comparison of Plato's and Polybius' views on the origin of society makes me regret the more that he has not contrasted fully Polybius with Aristotle on the mixed constitution. P., as he shows, took too little account of social factors and failed to see that no constitution can survive if a very important part of the population has no interest in its preservation (296). Aristotle had anticipated such a criticism; less concerned with constitutional forms, he suggests a genuine social compromise, to take account of the interests both of property and the masses, and allows that this may only be workable with a large middle class, mediating between rich and poor (such as did not exist at Rome).

Von Fritz says that it came as a revelation to P. that Rome's power was due to her constitution (p. 30). But to Greeks in general the *politeia* determined what a city was; it was then the source of its power (Hdt. i 65; Thuc. i 18; Xen. *Resp. Lac.* i 1 on Sparta; Hdt. v 78 with vi 131; Thuc. ii 36, 4 on Athens) or weakness (Isocr. vii 12 on Athens). (National character might be associated with the *politeia* as in Pericles' Funeral Speech, laudations of Sparta, and in Polyb. vi 47, 1; 53, 6, etc.) It was almost inevitable that P. should look first to the Roman constitution, and also, confronted by its *ποικιλία*, should take it to be a model instance of the mixed constitution; the most powerful state must have the best constitution, and Plato, Aristotle, the Stoics (Diog. Laert. vii 131) all agreed the *ποικιλία* was the best practicable. Hence P.'s treatment is (as von Fritz shows) schematic and often unrealistic. Each admirer of the mixed constitution fitted facts to his own Procrustean pattern. The *Menexenus* gives us one example, Blackstone in modern times another. Sparta, stable and powerful, had to conform to the ideal; the ephorate, ignored by P., could then be variously regarded as democratic (*Laws* 692a; *Pol.* 1265b 33; 1270b 17), oligarchic (Archytas? *ap. Stob.* iv 1, 138) or 'tyrannic' (*Laws* 712d; *Pol.* 1265b 40). The first view finds an analogy in P.'s over-estimate, which von Fritz (301) takes too seriously, of the democratic roles of the tribunes, 'mancia nobilium'. So, too, the fundamentally oligarchic feature of the Spartan system, the exclusion of most Lacedaemonians from citizen rights, is always ignored, just as P. ignores the timocratic organisation of the *comitia centuriata*. In general, P.'s observation was directed, and limited, by the framework of Greek political philosophy. Hence, despite ii 24, he forgot Rome's resources in manpower, in which Philip V, a less doctrinaire observer, had seen the prime source of Rome's strength; and he had no eyes for that liberality with the citizenship and skill in organising her allies which marked Rome off from Greek *poleis*.

With all the defects he sees in it, von Fritz claims for P.'s theorising an important *Nachleben* in modern times. Macchiavelli, he notes, drew heavily on it, but hardly perhaps for a very significant part of his own thought. Vain repetitions of P.—or Aristotle—on the mixed constitution had little relevance in an age of large territorial states, when religion was more often the main issue than social tension. In and after the seventeenth century we find a new doctrine of the balanced constitution. It shares with the old a distrust of power, abused if unrestricted, and employs some of the old terms. But it differs radically: (a) in form; (b) in purpose. (a) The separation of powers is very different from Aristotle's balance of interests, or P.'s fusion of monarchic, aristocratic and democratic institutions. Montesquieu's *Etat modéré* is the British constitution, as he understood it (*Esprit des lois* xi 6); his analysis even of the Roman system, in which he found another exemplar, relates to the distribution of powers, with small reference to P. (xi 12-9). True, his view that there should also be monarchic, aristocratic, and democratic elements in the balanced constitution seems to recall ancient theory, but (i) they play precisely the roles of King, Lords, and Commons in England; (ii) this was no essential part of his doctrine, and was easily discarded in America, when it was put into practice. (b) Ancient theorists praised the mixed constitution for ensuring stability, power, a just inequality. Montesquieu and his followers held that a balance of powers was required to preserve liberty. This was a new note: only democrats in antiquity prized liberty above all else. The *eleutheria*, long preserved in P.'s view by the Lycurgan system,



was surely, *contra* von Fritz (109), the independence of Sparta, not the liberties of Spartans. Cicero (*de rep.* ii 57) held that the mixed constitution at Rome gave the people 'satis libertatis'—to make them content, and prevent revolution; liberty was not the prime aim. The ancient theory was a reaction to *stasis* and radical democracy, the modern to Stuart prerogative and Bourbon despotism. In the eighteenth century autocracies could guarantee power abroad, and apparently order at home, but at the peril of individual freedom; but now that the equation of *νομος* and *ἀνδρα* no longer seemed applicable, and few could share Burke's feeling that the state was 'a partnership in every virtue and in all perfection', individuals asked to be left alone by governments over which they had so little control. Classical thought may colour the new doctrine: its effective influence cannot be asserted without close investigation. It would have been valuable to have had more than an *obiter dictum* from von Fritz. But Polybius warns us to judge an author by what he chooses to give, not what he omits; von Fritz has given us much, and we owe him our thanks.

P. A. BRUNT.

**Apokrimata. Decisions of Septimius Severus on legal matters.** Text, translation, and historical analysis by W. L. WESTERMANN. Legal commentary by A. A. SCHILLER. New York: Columbia University Press, 1954 (London: Geoffrey Cumberlege). Pp. x + 111, with 1 facsimile. 60s.

The joint editors of this important text can hardly be said to have co-operated. Professor Westermann transcribed the papyrus, wrote a translation, and added some historical notes, including a complete list of Severus' constitutions preserved in the papyri and a discussion of the date of Severus' visit to Egypt: all this evidently without consulting his legal colleague. Professor Schiller used Professor Westermann's transcript, but made a second translation of his own—wisely, since Professor Westermann's version is clearly wrong on many legal points. In his commentary he flatly contradicts his colleague on a number of points. Thus, Professor Westermann, on the *a priori* ground that Severus and Plautianus must have known Greek, declared that the document was originally written in that language. Professor Schiller proves with a wealth of examples that it is a translation from the Latin. This is hardly a satisfactory way of publishing an important document, but to make matters worse, it has subsequently transpired that Professor Westermann's transcript of the text was wrong on a number of important points. A corrected text has now been published by Professor Youtie in *Chronique d'Égypte*, XXX (1935), 327–45; and in the same place Professor Schiller has made consequential changes in his legal commentary—amounting in two cases to a new treatment of a quite different legal problem from that posed by the faulty original text. The reader must therefore use the book only as corrected by the article. In this review the revised text and commentary will be discussed.

The document consists of thirteen rescripts (or *subscripta*) of Severus and his elder son, posted in the stoa of the gymnasium at Alexandria between 14 and 20 March 200. The rescripts concern a variety of topics, administrative and legal, and, as they are very laconic and the *libelli* which they answered have not been copied, offer a wide field for conjecture. Professor Schiller has shown great ingenuity in reconstructing the situations to which the replies must refer, and great learning in working out the legal problems involved, making use both of the standard texts of Roman law and of the papyrological texts which illustrate the local law of Egypt. Among the legal problems discussed are the sale of mortgaged property on foreclosure, the guardianship of orphans, the renunciation of an inheritance by a minor, the legal capacity of women, and the order of proceedings when a will was contested as a forgery. Of administrative interest are rulings on hereditary Egyptian priesthoods, and illness as an excuse for liturgies. To the historian the most exciting rescript is that in which the emperor states that his praetorian prefect, Flavius Plautianus, will investigate the criminal activities of a certain Comon. There are also some tantalising scraps of evidence on the vexed question of judicial procedure in the provinces. The phrase *ἐξωθεν πάσης* (*extra ordinem*) occurs in one rescript, apparently in connection with the appointment of guardians. In another an official summons of the accused is promised if the question (alleged forgery of a will) is *ἐν τῇ τάξει τῶν διαγνώσεων* (*in ordine cognitionum*). On the other hand, in a civil action it is stated that *δικαστὴς δόξαται* (*iudex dabitur*), and in another case the emperor promises that if the defendant (a tax-farmer) is not involved in Comon's criminal activities as an accessory, the plaintiff will have the governor of the province as *iudex* (*δικαστὴς*), presumably in a civil action. The language of the rescripts seems to imply a distinction between *cognitio* in criminal trials, and *iudicis datio* in civil actions, but is too vague to prove it.

Professor Schiller is perhaps sometimes too legalistic and ingenious, presuming in petitioners a knowledge of law which they probably did not possess. To judge by many rescripts preserved in the *Codex Justinianus*, petitioners often asked the most elementary questions and stated the issue in a very confused way. May not the fourth rescript be a reply to a rambling complaint that the creditor had sold the mortgaged property and violently taken possession of the debtor's lands, and enunciate the two simple principles that on the one hand the sale of pledges cannot be rescinded, and that, on the other hand, the governor will restore to the owner possession of land occupied by violence? In the twelfth rescript the situation also seems quite simple. The petitioner has renounced his inheritance and it has been confiscated—there must have been fiscal claims against it exceeding the assets. He has no claim to *restitutio in integrum* as a minor—presumably the time limit for making such a claim had run out. He must therefore obey τῷ νόμῳ τῶν πράσεων. This surely cannot mean the *Lex Aesetia*, but the fiscal rules governing the sale of confiscated property.

A. H. M. JONES.

**De Phylakieten in Grieks-Romeins Egypte.** By P. KOOL. Pp. x + 133. Amsterdam: Studentendrukkerij 'Poortpers', 1954. Price not stated.

This doctoral dissertation is a thorough and workman-like examination of all existing evidence for the activities of the phylacites in Ptolemaic Egypt, in the course of which the histories of the phylacites, archiphylacites, epistates of phylacitae, and subsidiary officials, together with the *phylaciton*-tax, are carefully traced. There is also a general introduction on the function of police in Greece and Pharaonic Egypt and on the sources utilised, a brief conclusion and summary (in English), and the usual notes and indexes. All known references to the phylacites are examined in their context, and the strands of information so acquired drawn together into a coherent pattern.

Kool inclines to the view that Ptolemy I, or even Alexander, adopted the office of phylacites after finding it as an already existing institution in Egypt; the office might be held by Greeks or Egyptians, but the possibility that Arabs also held it has, since the publication of this volume, been exploded once for all by van Groningen in *Mnemosyne*, IV, VIII<sup>1</sup>, 1955: Ἀραβὸν φυλακίτου in P. Hamb. I, 104 should read ἀραβὸν φυλακίτου. Phylacitae were found in the towns and villages of the χώρα but not in the Greek *metropoleis*. They were responsible, first and foremost, for the maintenance of security and public order, the arrest of offenders, and the investigation of the evidence against them. In the second century B.C. we find the *epistates phylaciton*, who heads the police-organisation of the nome, assisted by the archiphylacites and a secretary, himself trying and judging cases involving private delicts, which in the third century he would have referred to the strategus, but there is no trace of the employment of the archiphylacites in a similar capacity. Apart from this responsibility for private property and for offences against the state, the phylacitae were also commissioned to act for officials in charge of various departments, their oldest and most important task being the custody of the crops. Again, they might be employed as escorts for transport by land and sea, in the first instance state-owned only; possibly, they were also instructed to guard the royal pastures and to collect grazing fees. Kool tends to connect the phylacitae with καταλοχιστῆς, whom he regards as a special branch organised on military lines for police-actions on a larger scale and for special services, with the 'cleruchs among the phylacitae', the cleruchy having been introduced for the benefit of phylacitae by Ptolemy Philopator, so that it was possible, at least by the end of the second century B.C., for Egyptian phylacitae to become κάτοικοι and so gain Greek nationality: these 'cleruchs among the phylacitae' had a separate organisation, perhaps even their own *epistatae* and *grammateis*, and possibly a place can be found in their number, as commanders of groups in military squadrons, even for the mysterious *decani*.

An analysis of the royal ordinances recently edited by Turner and Lenger in P. Hib. II will no doubt enable Kool to throw even greater light on the police-organisation of Ptolemaic Egypt. As it is, he sees little development in the office of phylacites: its importance and authority increased in the second century B.C., only to disappear almost entirely under the Roman administration. The archephodus displaced the archiphylacites in the village; only the *epistates phylaciton* remained, probably because of his judicial authority. Kool refuses unqualified belief to Wilcken's view that the Roman centurion succeeded the *epistates phylaciton*, but admits that it was the arrival of a new army of occupation which was the principal cause of the disappearance of the police-officials of the old, though the land and taxes of the phylacitae are still mentioned in the third century A.D.

B. R. REES.



**The Acts of the Pagan Martyrs, Acta Alexandrinorum.** Edited with commentary by H. A. MUSURILLO. Pp. xiii + 299. Oxford: Clarendon Press, 1954. 35s.

Those strange documents, commonly known as the Acts of the Pagan Martyrs, which cast so tantalising a half-light upon the question of provincial criticism of the Roman Empire and the Emperors, have long been awaiting synoptic collection and discussion. Here we have it, together with the latest additions to the series of texts.

The characteristic of this book is balance, a quality hitherto sadly lacking in many studies of the Acta. Musurillo has a fine sense of literary values, and picks his way with care through the snags and pits into which earlier scholars have often fallen. For him the Acta derive ultimately from official reports, which have been transformed into a new kind of journalistic literature with a special purpose. They have elements common to novel and mime, but are neither romance nor drama. They show traces of Cynic vocabulary in the abuse of government, but their authors' principles are the very reverse of Cynic. They may owe something to the late Hellenistic and Roman literature of heroic deaths, but their resemblance to Christian martyrologies is superficial and not derivative. Their anti-Semitic tones are due to the Alexandrine environment, but are not their core and principle. They do not form part of a continuous work nor even of a single collection.

So far M. trims and adapts the luxurious theories of his predecessors. Then his solution. The Acta are the product of the affronted pride of the aristocratic class of Alexandria, a new species of tract reflecting the indignation of the gymnasiarchic families at the humbling of Alexandria by Rome, written mostly in an anti-Roman political spirit, but sometimes for simple mocking entertainment. They always reflect the prejudices of their class, some members of which were drawn from the Roman provincial hierarchy itself and hence less critical of Rome than were others. The Acta ceased to be written (though not to be read) when Rome made final peace with Alexandrine civic pride in the time of Septimius Severus.

This judicious view is set forth in a series of scholarly appendices in the second part of the book. M. has missed little in his gleaning, and has not feared to challenge the conclusions of great names. He might perhaps have considered, as an analogy rather than an influence, the satirical account of Claudius in the form of a *Cognitio*, like the Acta, enshrined in the Apocolocyntosis. But all in all he gives the best solution yet of the problem of the Acta.

The first and longer part of the book contains a publication, with full palaeographical and historical commentary and partial translation, of everything that may be considered an Acta-text, and of other relevant documents such as the Boule-papyrus. The sphere for disagreement is vast, but again the judgement of M. tends to bring him out on the side that commands the greater probability of truth, though he writes with less authority on matters of history than about literary processes. The reviewer, who saw this work at an earlier stage, was greatly impressed by the deepened understanding of Roman history shown in the final version. The interpretation of the historical setting of the Trajanic and Hadrianic Acta is very apt. The curious assertion that Trajan's cabinet was packed with Jews is ingeniously referred to the group in the Acta Hermaisci of personages, Agrippas and Alexanders, of Jewish descent. M. wisely refuses to press vague references in Dio Chrysostom's orations to fix an exact date for the Hermaiscus incident, but he misses possible evidence for dates of composition in the suggestion in these and other Acta that an Emperor should not torture a notable of Alexandria. The assumption about the privileges of *honestiores* indicates a date for such documents not earlier than the reign of Hadrian, when the privileged position of this class first began to be established.

In dating the dramatic moment of the Acta Pauli, with its possible reference to a Jewish revolt, to Hadrian's first rather than Trajan's last years, too much is based on the restoration of the name of Q. Rammius Martialis, the known Prefect, from the three letters  $\mu\alpha\tau$ . The only Prefect certainly mentioned, and that twice, is Trajan's last man, Lupus. But M. may be right for a reason which he does not give: if the trial is to be located as in all the other Acta at Rome, only Hadrian is possible.

As for the Acta Athenodori, one may still doubt, despite M., whether the Epistula Caesaris, with its rhetorical and philosophical overtones, fits Trajan better than Hadrian; compare the careful characterisation of Marcus and Commodus in the Acta Appiani. Musurillo feels his way carefully through the prosopographical mazes of the latter Acta, and should be right in rejecting Von Premerstein's identification of the aged man Heliodorus with the son of the rebel Avidius Cassius.

At times a certain timidity is apparent which leads to unduly long notes, as in the laborious discussion of the phrase  $\epsilon\lambda\iota\sigma\tau\epsilon\sigma\iota\varsigma$ , clear enough in its context. Hence, too, his failure to

clinch his case boldly in dealing with the Acta Maximi. Here, if ever, we have the circulation of the report of a trial and attendant speeches—the trial of a Prefect—written up beyond the measure of the official commentary, but with no such propagandist or romancing tendency as is apparent in the other Acta—simply because it was of great interest to the Egyptian public. This gives the surest evidence of the ultimate dependence of the Acta on official documents. M. takes the view that the historical core of narrative in the Acta is generally derived from Roman archives and brought to Alexandria. But the emissaries who attended the trials could report and circulate just such an account as we have in the Acta Maximi without ransacking the archives. Not all of them perished at Rome.

The new fragment of a speech, here called Acta Diogenis, is cautiously handled by M. His identification of the 'old-fashioned emperor' as Vespasian is evidently right in the context, which mentions Nero, and whether Diogenes is the critic of Titus or not, he certainly belongs to the Cynic gang. But the worst problems are those attending the older group of Julio-Claudian documents from the Boule-papyrus to the Acta Isidori. Rejecting the fantasies of Von Premerstein, M. sets the problems out in all their difficulty, and seeks to solve only what can be solved. He argues for the literary character, historical content, and Claudian date of the Boule-papyrus, stressing the familiarity of the document with Roman administrative methods. The Interview with Flaccus and the Gerousia Acta remain an enigma even to M., who suggests, not wholly convincingly, that the former contains an account of extortion *flagrante delicto*, and can find nothing certain in the latter except the existence of the Alexandrine Gerousia. He prefers to minimise the historical content of the Isidori Acta, but does not shirk their complicated prosopographical problems, and cautiously reinforces the case for dating the trial of Isidore to the latter rather than the first years of Claudius. His tentative suggestion that the puzzling silence of Josephus about this affair was a diplomatic effort to protect the reputation of his patron Agrippa II is not implausible. But he perhaps underestimates the weight to be given to the appearance of Balbillus in the trial. If this is the known equestrian personage, then the circumstances of his busy career may favour the earlier date rather more strongly than M. allows, especially if he intervenes in a private capacity.

But in general scholars are more likely to agree than to quarrel with M.'s numerous discussion of details. Altogether this commentary and reconstruction is a remarkable achievement for a young scholar. The texts themselves have been prepared with the usual care and characteristic caution of Musurillo, who admits even the most convincing supplements only upon sufficiency. But better qualified scholars than the reviewer are known to have been greatly impressed by the quality of M. as a papyrologist.

A. N. SHERWIN-WHITE.

**Achaean and Indo-Europeans. An inaugural lecture delivered before the University of Oxford on 4 November 1954.** By L. R. PALMER. Pp. 22. Oxford: Clarendon Press, 1955. 2s. 6d.

Professor Palmer has given himself the fascinating task of helping to interpret the language of the Pylos tablets, which were discovered by Professor C. W. Blegen at Ano Englianos (Morea) in 1939, deciphered by Mr. M. Ventris in 1952, and identified as a Greek dialect of the eastern or Arcado-Cyprian and Aeolic type by the latter and Mr. J. Chadwick in a paper published in the *JHS* in 1953. A tentative and fragmentary account of the structure of this dialect, called 'Mycenaean' or, preferably, 'Old Achaean' by Mr. Chadwick, figures in the *TPS* (1954) and is followed there by Professor Palmer's own paper 'Mycenaean Greek Texts from Pylos', which appears to be the basis of the present lecture. This is presented to the reader in a pleasing and elegant style, with few lapses into the crude terminology of modern linguistics, and offers a personal vista of Mycenaean polity by drawing on Homer and the cumulative and disparate evidence of Hittite, Vedic, Italic, and Germanic. Professor Palmer's picture shapes itself out of a 'dark tangle of conjecture' and is incomplete, to be sure; but what is illuminated reminds us forcibly of the plausible and abundantly illustrated arguments of Otto Schrader's graceful essay *Die Indogermanen* (Leipzig, 1919). We should, however, notably fail to keep faith with the best interests of scholarship if we merely accepted Professor Palmer's hypotheses without demur, and it seems obvious to me that he does not intend us to do so. The transparency of his method of playing 'Indo-European themes across this defective instrument (the Pylos texts)' to see whether it responds intelligibly to an Arcado-Cyprian idiom shows that we are on very uncertain ground here and that perhaps too much scope is left to surmise. But the task was surely worth doing, for in the end, if Mr. Ventris and Mr. Chadwick are right



in their main contention, the facts now hidden or distorted may later become evident. As Professor Palmer's knowledge of the Indo-European community and its stratification depends on what, after all, is a laborious reconstruction out of the most diverse evidence, he is naturally led to follow the only course available and interpret the unknown through the known. The twist which this gives to the new data results, for instance, in imposing on the organisation of Mycenaean Greece the familiar features of the Germanic feudal system, which inevitably upsets established chronology. But Professor Palmer's enthusiasm for his subject is irrepressible and infectious: 'I venture (he says) to believe, too, that through the now open door of Nestor's archives room we can descry, at least in broad outline, the polity of the Indo-Europeans.' We share his faith, for he has scholarship and imagination, and, best of all, uses both with due modesty.

W. K. MATTHEWS.

**État actuel de l'interprétation des inscriptions créto-mycéniennes.** By V. GEORGIEV. Pp. 76. Bulgarian Academy of Sciences, Sofia, 1954. In Russian, with French summary.

**Lexique des inscriptions créto-mycéniennes.** By V. GEORGIEV. Pp. 96. Bulgarian Academy of Sciences, Sofia, 1955. In Russian.

The present Rector of Sofia University has been known since 1937 as a leading exponent of the theory that evidence can be found, in Greek vocabulary and place-names, for the existence of an earlier pre-Hellenic or 'Pelagian' language, non-Greek but nevertheless Indo-European. 1949-53 saw the publication of three books in which he announced the decipherment of Linear B in terms of such a language; although stated to belong to the 'Aegean-Asiatic' group together with Hittite, the reconstruction of many of its words and inflections was argued from Greek models. It is to Georgiev's credit that he was prepared to consider a Greek element in Linear B at a time when it was unfashionable; but in view of more recent developments it requires stating that only one of his suggested interpretations (that of *πάλλω*, already made by Evans) agrees with the vocabulary now revealed, that the process by which he arrived at them appears to follow a completely different logic, and that to postulate a language which is at the same time closely related both to Hittite and to Greek shows some disregard for the gentlemanly rules of the game.

His 1954 book is an extended review of Ventris' and Chadwick's article in *JHS* LXXIII (1953), in which somewhat uncoordinated attempts are made both to show the improbability of its conclusions and to reconcile them with his own earlier interpretations. It ends, however, with the admission that the Linear B texts are specifically Greek, and that the attack on the 'Pelagian' language must now be transferred to other fronts.

The second book is a lexicon of most Greek interpretations of Linear B words made by scholars up to early in 1955: Georgiev's own additions to these show his now more or less complete conformity. The lexicon is intended as a preliminary to the publication of some 300 texts in translation, similar in scope to Ventris and Chadwick's *Documents in Mycenaean Greek*; like Meriggi's *Glossario miceneo* (which appeared almost simultaneously), it does not yet include any of the numerous tablets found at Pylos since the war. (Words drawn from the new tablets published by Bennett have now been included in the second of two supplements to Georgiev's *Lexique*, dated 1956.)

Both books contain a short digression into the Linear A tablets from Agia Triada, which 'also contain the Greek language': a conclusion which excites the gravest doubts.

MICHAEL VENTRIS.

**Studies in the Language of Homer.** G. P. SHIPP. Pp. x + 155. Cambridge: University Press, 1953. 18s.

In spite of its modest form, this is an important book. Professor Shipp first argues that *-ai* began its career as an instrumental singular and developed its other Homeric uses during the formation of the poems. The case is convincingly argued, and it is bad luck that it has so soon been challenged by the evidence of the Linear B Tablets (*Trans. of the Phil. Soc.*, 1954 (1955), p. 11). The limitation of *εἰσοφα* to similes is, however, an insignificant part of the analysis of linguistic peculiarities, which is his main subject. He examines the passages noticed in Professor Chantraine's *Grammaire homérique I*, thus avoiding any suspicion of biased selection, and concludes that in the *Iliad* neologisms are disproportionately frequent in similes and other passages which fall outside the main narrative: digressions, biographical details, descriptions, and comments. Clearly a good deal of the poem could come under these headings, especially as comment in speeches is included, and we need

some estimate of the number of lines concerned if we are to compare the neologisms listed as from the narrative (hereafter *narr.*) with the slightly larger number in non-narrative passages (hereafter *misc.*) which have to be extracted from Chapters II, III, V, and VI. An attempt to make the calculation showed at once that the two types of passage are inextricably connected by sense and syntax, and indeed the author explains several neologisms in *narr.* by disturbance of the narrative to make room for a simile; but very roughly it seems that about a quarter of the poem should be considered *misc.*, and that in it neologisms are proportionately about four times as frequent as elsewhere, while in *narr.* there is about a quarter in which they are about three times as frequent as in the rest, and which is therefore almost as 'neologistic' as *misc.*, A 1-52, *Catalogue*, K, Olympian scenes in Θ and O, Machaon scenes in Λ, T, and from the death of Hector to the end. The neologisms in K are exceptional in quality as well as quantity. There are a few surprises; Nestor in Λ is not more neologistic than the poet of A 1-52, and some scenes not generally thought to be the earliest, such as the *dream* and the *hortatio*, are markedly free from neologisms.

The author himself is cautious in drawing general conclusions, disclaiming (Socratically, one suspects) adequate knowledge of the literature of the Homeric Question; but since his results are already being quoted in support of a Peisistratean 'poet of the similes', it is important to note what the evidence does not prove. The cumulative effect of the examples establishes relative, not absolute, lateness. Certain Atticisms and usages later than surviving documents are very rare, and must still be considered individually. It may be doubted if the new always superseded the old immediately (some English-speakers still use 'gotten'), or if, for example, *Ἀγροῦς καὶ Διὸς οἶος* (p. 134) is adapted from the *Hymns* or from a formula older than both. In any case, they are not particularly localised, and their number is too small for the statistical method. What does seem to be established is that neologisms are needed four or five times more often to express the thoughts, actions, and ornaments which make the poem into the *Iliad* than to narrate routine activities such as battles, journeys, meals, and oaths, the main themes presumably of earlier poetry, for which the traditional language sufficed.

The statement of the evidence is clear and objective. There are good comments on such subjects as the influence of metre, the adaptation of formulae, the development of similes, and the unwisdom of emending words in *misc.* in conformity with usage in *narr.* It is to be hoped that the author will give us more, with more emphasis on his qualitative groupings of neologisms (p. 19), and a further analysis of different kinds of non-narrative passages.

D. H. F. GRAY.

**Sprachgeschichte und Wortbedeutung. Festschrift Albert Debrunner.** Pp. 474. Bern: Francke, 1954. S.Fr. 60.

This *Festschrift*, consisting of nearly forty essays presented to A. Debrunner in honour of his seventieth birthday by pupils, colleagues, and friends, is an appropriate tribute to the fertile and many-sided learning of a scholar, the list of whose publications from 1907 to 1954 inclusive contains almost 600 items. The number and diversity of the contributions make a critical review of them impossible; what is attempted here is no more than to display the riches of the volume, in the hope that its very character will exempt the reviewer from the condition laid down by J. Whatmough, that 'a review, to be worth recording, should possess the distinction of adding something to the subject' (p. 442).

Only one article has no reference to linguistic questions—that of O. Gigon, who examines the treatment of the foundation legends by Roman historians, with special reference to the various accounts of Numitor, Amulius, and the birth and exposure of the Twins; M. Leumann's essay on the relation of the Church Slavonic Kiev Manuscript to its Latin original is of interest almost exclusively for Slav and ecclesiastical history. Of the remainder it is advisable, since this synopsis is intended for the classicist, to mention first those articles which are not relevant, or not specially relevant, to classical studies: G. Deeters's classification of expressions for 'to have' in Georgian, F. Edgerton's semantic notes on Buddhist Hybrid Sanskrit, K. Jaberg's study of the words for 'sling' (*Schleuder*) in the Romance languages, F. B. J. Kuiper's notes on two Rigvedic loan words, C. Regamey on the ergative construction in modern Indo-aryan, L. Renou on the development of the infinitive in Vedic, and a methodological study, using Baltic material, by A. Senn. Three contributions concern the Indo-European field as a whole: G. Bonfante discusses and illustrates the rôle of animist conceptions in Indo-European languages ancient and modern; H. Krahe, representing onomastic studies, shows that



the original forms of *Oder* and *Eder*, as of many other river-names, including some in Italy, go back to a period of Indo-European earlier than the 'crystallisation' of the historical languages; W. Porzig contributes a semantic study of the relations between words for 'old', 'young', and 'new', suggesting that their distribution in various Indo-European languages has geolinguistic and sociological implications. A third group may be made of those essays which will interest mainly the specialist in classical linguistics. Among these are two new etymologies: E. Benveniste detaches *μνᾶσθαι* 'to seek a wife' from *γυνή*, etc., and identifies it with *μνᾶσθαι* 'to remember, mention', adducing such expressions as *mentionem facere* (*de filia*), which he thinks may be calqued on the Greek; F. Sommer sees in Skt. *sákthi* for *\*sákthi* a formation on the analogy of *ásthi* from I-E. *\*sǵeng*, to Germ. *Schenkel*, etc., Gk. *σκέζω*. In morphology and word-formation A. Bloch makes an interesting case for supposing that the Latin suffix *-id(us)* arose from adjectives in *-ro-* by dissimilation of *r* to *d* when *r* occurred in the body of the word, and analogical extension of the resulting *d*-suffix; J. Kurylowicz, in a characteristic blend of neo-grammatical and structural methods, argues from Germanic, Slavonic, and Sanskrit parallels that the *-ō-* of the comparative *-ώτερος* is the adverbial *-ω(s)*; M. Leroy shows the untenability of the traditional classification of *ζῶ*, *πνέω*, *διδῶ*, *χρῶμαι* as 'verbs in *ā* contracting in *η*'. The only essay in classical syntax is that of M. Lejeune, who shows with examples that the distributive use of accusative and genitive, as of various prepositions, is a specialisation of their use to denote pure abstract relation or reference. The remaining contributions are in various ways relevant also to fields of study other than linguistic. G. Devoto argues that *ἀπολογία* is an old *nomen agentis* denoting, like *πρόξος* and others, not a person but an object, viz. a milking-pail or other receptacle of similar form, whence, on the analogy of its apparent shape, the (night) sky. In the realm of word-formation M. Niedermann, in an article on *-inus* as a diminutive suffix in late Vulgar Latin, advances some interpretations of epigraphical and literary texts; E. Risch's explanation of Homeric nominatives masculine in *-ῶ* as developed by extension from two prototypes, originally vocative (viz. an epithet of Zeus, perhaps *μητέρα*, and *ἡπτότα* as an epithet of Nestor) fits into the wider question of internal imitation in Homeric poetry. Also of interest to Homeric scholars is J. Friedrich's collection of Hittite and Proto-Hittite texts in which the languages of gods and of men are contrasted. The only ethnographical article, that of W. Brandenstein, on the identification of the peoples listed in *Genesis 10*, is in part relevant to the history of the earliest Greek settlements in Cyprus and South-east Asia Minor. Of the semantic studies, which form the largest group of contributions, almost all are on Greek or Latin subjects, and are of importance for the understanding of texts. P. Chantraine and O. Masson collaborate in a study of *δῶρος* and its derivatives (maintaining their relationship to *δῶρος*, *ἐξορία*) which throws light on the history of Greek religious thought. W. Havers illustrates from several languages, including Greek and later Latin, the specialisation in sacred contexts of one of a pair of synonyms. O. Hiltbrunner shows *gravis*, *grauitas* as terms for a characteristically Roman ethical concept, originating in the forensic sphere and later revived by Christian writers. H. M. Hoenigswald explains the use of *μῦθος*, *medius*, to denote the voiced plosives as due, not to an attempt to describe the sounds, but to a kind of terminological laziness, like the original use of *τό μετὰ* for the neuter gender and of *μῦθος* for the circumflex accent. Two articles are concerned with Hebrew influences on Jewish and Christian Greek; those of W. Michaelis on *πρωτότοκος*, of which the meaning 'specially favoured' is shown on the evidence of the Septuagint to derive from the Hebrew *b'kōr*, and of C. Mohrmann, who examines the history of *δόξα* in Christian Greek, especially its acquisition of the meanings 'visible glory', etc., from Heb. *kabod*, and its corresponding temporary loss of the meaning 'opinion'. G. Redard seeks to account for the diverse senses of the derivatives of *\*dek* in Greek (*δίκωμαι*, *δοκέω*, etc.), Latin (*deceat*, *docere*), Hittite and Sanskrit from a basic meaning 'se conformer à ce qu'on considère comme une norme'. M. Scheller, in maintaining that the formally identical *γνήσιος* and Skt. *jātya-* may, in view of the semantic discrepancies between them, be independent parallel creations, considers the meanings of both words and the contexts of ideas to which they respectively belong. H. Seiler reviews a number of Homeric passages in arguing that the original meaning of *δόσαντο*, *ἔτη* was 'act(ion) in error'; other meanings, as well as the active and the aorist passive, are secondary developments. Less easily classified but equally interesting are some 'Lese Früchte' garnered from Homer by E. Lewy, including observations on the not infrequent conjunction of active and middle verbs, such as *διείρει* *ἡδὲ μεταλλῆς*, *ἀγορεύσαντο καὶ μετέπειν*; and a review by W. Theiler of the views of ancient philosophers and theologians,

pagan, Jewish, and Christian, on the problem of the language used in divine utterances.

The volume is, therefore, in the main classical in interest and philological in subjects and method. There are, indeed, only three essays on aspects of General Linguistics, those of O. Funke on form and meaning in language structure, W. Henzen on 'Wortbedeutung und Wortnatur' and—in a very different field—J. Whatmough on the prospects before statistical linguistics. G. Redard supplies, besides the contribution already mentioned, the preface and list of Debrunner's publications. He and his colleagues deserve congratulation for a volume which in the range and proportion of the subjects it treats so closely reflects the interests of the distinguished scholar whom it honours.

D. M. JONES.

**Geschichte der griechischen Sprache. II, Grundfragen und Grundzüge des nachklassischen Griechisch (Sammlung Götschen, 114).** By A. DEBRUNNER. Pp. 144. Berlin: De Gruyter, 1954. DM. 2.40.

O. Hoffmann's *Geschichte der griechischen Sprache* in its first and second editions formed a single volume of *Sammlung Götschen*. Professor Debrunner, in preparing its third edition, excluded from it all matters belonging to the post-classical period of the language, which was to be reserved for a separate volume. Consequently the third edition of Hoffmann (or, as it must now be called, Hoffmann-Debrunner) is now the first volume of a two-volume history of the Greek language, of which the second, projected but never written by Hoffmann, is an entirely new work by Debrunner and owes no direct debt to the former scholar.

An introductory chapter sketches the history of post-classical Greek studies from the second century A.C. to the present day, and deals briefly with the upper time-limit of the post-classical language and with the various terms applied to it by writers both ancient and modern. The bulk of the book is divided into two main parts. The first, 'Grundfragen des nachklassischen Griechisch', opens with a description of the most important primary sources and goes on to an account of the political, cultural, and social conditions which determined the origin of the Hellenistic *Koine* and its spread in the Greek-speaking world. Since the beginning of post-classical Greek is to be sought in the classical period, Debrunner does not hesitate to epitomise certain aspects which, regarded from a merely chronological viewpoint, might seem to belong to the first volume. In so doing he makes it clear by implication that this second volume is more than a continuation of the first; that it is, in fact, an independent and self-contained work. There follows a particularly interesting section on the relation of the *Koine* to the old dialects and on the different conditions and rates of 'Koinisierung' in various dialect areas, including Attica. In view, however, of the special relation of Attic, Ionic, and *Koine* as set out by Debrunner in, e.g., §§ 95 ff., what is meant by asking, in the case of an Ionic word in late fourth-century Attic, 'ob es aus dem Ionischen oder aus der *Koine* nach Attika gelangt ist' (p. 45)? The general impossibility (which Debrunner notes) of answering this question is surely *prima facie* evidence that it is inadequately formulated, since this traffic in vocabulary between Ionic and Attic is one aspect of the emergence of the *Koine* itself. Nor is it likely that, as stated on p. 46, *ἡσθη* in an Attic inscription of the mid-fourth century is a blend of Attic *ἡτη* and Ionic *ἡσῶ*; it is rather an adaptation of the Attic word to the phonetic usage of Ionic and the *Koine*, which would have occurred in all probability had Ionic *ἡσῶ* never existed. A similar absence of precision in visualising the relation of *Koine* and dialect is to be seen in the discussion of 'hyperdialectisms' (p. 50), in which no distinction is made between the correct adaptation to dialect form of words borrowed from the *Koine* (such as *Θεσ. αναλογία*) and the incorrect and purely artificial use of phonetic features felt to be peculiarly characteristic of a dialect (such as 'hyperdoric' *ᾶ*); these two types, though both indicate external influence on a dialect in which they occur, permit very different conclusions about its vitality.

Varieties of West Greek *Koinai* are briefly described in pp. 47-8, but the part obviously played by the Attic-Ionic *Koine* in their creation is not mentioned. Considering the contribution of dialects other than Attic and Ionic to the *Koine*, Debrunner seems at times to overestimate their influence on its phonetic and grammatical features. West Greek need not be invoked to account for the Hellenistic gen. sing. in *-ᾶ* of some first-declension masculines, nor Boeotian to explain itacism and related phenomena. A further section describes the expansion of the *Koine* beyond the Greek homeland, and the tribute it took from foreign languages, especially Latin. In the paragraph on Greek in Syria there is, however, nothing in the passage cited from *Peregrinatio Aetheriae* to support Debrunner's statement that in Syria readings from the Greek Scriptures were translated into



Latin as well as Syriac for the benefit of the congregations. A short chapter discusses the relation of linguistic and rhetorical Atticism, and illustrates the character of the former with examples from Atticist and anti-Atticist lexica.

The second main part, 'Grundzüge des nachklassischen Griechisch', summarises the characteristics of post-classical Greek under the usual heads of phonetics, morphology, and syntax. Again Debrunner is inclined to the view which accords to Boeotian an important part in determining the pronunciation and phonetic development of the *Koine*; but cultural and chronological considerations are opposed to this explanation of phenomena which in any case belong to the commonest types of phonetic change. In a book generally so well proportioned it is perhaps surprising that the question of vowel-quantity and accent is dismissed in less than half a page, while the syntax of the optative receives a treatment far more extensive and detailed than any other subject in this section, occupying no fewer than five pages; in comparison, the infinitive is stinted. The book ends with a short epilogue on the increase of interest in post-classical Greek since the first appearance of Hoffmann's *Geschichte*, and with no fewer than five indexes (ancient literature, modern literature, grammar, Greek, Latin), which make it more than usually easy for reference.

Debrunner has packed into this little book an extraordinary amount of information. Paragraphs in larger type giving an easily readable account alternate with others in smaller type and a more succinct and 'telegraphic' style which contain well-chosen illustrations and references. To each separate topic is appended its own bibliography, short but sufficient for further exploration. The book should be of the greatest service to two classes of readers. Those who, to satisfy a general interest, desire a short but not niggardly account of post-classical Greek will probably find in it as much as they need; prospective specialists will be grateful for so masterly a survey of the whole field and even, here and there, for the suggestion of a theme for research.

D. M. JONES.

#### Studies on Intensification in Early and Classical Greek.

By H. THESLEFF. Pp. 227. Helsingfors: Societas Scientiarum Fennica, 1954. Price not stated.

Thesleff's object, as he formulates it in his preface, is to study 'the process of adopting different words and locutions for the grammatical purpose of expressing a high degree' in Greek. He is conscious of the relation of this to other forms of expression, and promises a treatise on the Greek superlative and eventually a general treatment of the related problems with special reference to Attic.

The Introduction is devoted mainly to definitions of the notions to be studied and of the terms to be used. 'Intensification', the expression of a high degree, is distinguished as a particular aspect of the more general 'strengthening', the expression of various kinds of emphasis and prominence. Intensification can properly be applied only to 'concepts of relative value', which admit degrees, whereas 'concepts of absolute value', including totality and completeness, are susceptible of other forms of strengthening. This scheme is complicated in practice by the impossibility of drawing an absolute boundary between the two classes of concept in question. Besides this difficulty, which is anticipated in the Introduction, others become evident and receive comment later in the book, especially that of distinguishing the cases in which a particular locution retains an original qualitative value from those in which it has a purely intensifying function. By a further semantic weakening there is a common tendency for strengthening words to become expressions of mere confirmation or affirmation. Since some or all of these functions may be exhibited in appropriate contexts by the same word or phrase, Thesleff rightly insists on the importance of the part played by the context in each instance.

In the first part of his work Thesleff studies the occurrence in authors from Homer to the fourth century of the specifically intensifying adverbs *πάρα*, *πάνω*, *κάτω*, *οὐδέν*, *τοῦτο*. Each is treated according to a common plan: tables of statistics and references are followed by a brief comment on possible etymologies and original meanings; the intensifying use with various word-classes and in particular combinations, especially with negatives, as well as confirmative and affirmative uses, is exemplified and discussed; finally, conclusions are drawn about the meanings and stylistic value of the word in question. The second part of the book reviews the intensifying function of an impressive number of words, phrases, and grammatical forms, ranged in various classes, including the degrees of comparison, expressions of totality, and quantitative, spatial, and qualitative expressions. General conclusions are set out in the final chapter.

To verify fully so great a number of references and quotations would be a huge task; fortunately a partial check suggests that

it is also an unnecessary task. Thesleff appears to combine accuracy of presentation with soundness of interpretation, and a few slips, if there be such, could not imperil the whole structure. He admits in several passages, at least by implication, that some hair-splitting cannot be avoided in dealing with a subject of this kind. There is, for example, a valid distinction between verbs with which *οὐδέν* retains its qualitative value and those with which it is purely intensifying; but some verbs cannot be unhesitatingly assigned to the one class or the other. Thesleff has deliberately (§ 406) included some material which inspection may show to be irrelevant, rather than risk the omission of what may prove to be pertinent; there are times when his caution seems excessive. If one feels some doubt about his derivation of *πάνω* from *πάνω* *εἰς*, it is not that one feels satisfaction with previous suggestions. It would have been interesting to know what Thesleff has in mind when he speaks of stress in Greek (e.g. in §§ 47, 50, and especially 392). To say, in discussing the occurrences of confirmative *οὐδέν*, that 'the absence of examples from comedy is compensated by the Lysias-instance, and Demosthenes' is evidently something of an over-simplification. Such debatable points could easily be multiplied, and serve only to show how much of interest the book contains. It must be admitted that it is not an easy book to read; interesting points, such as the importance of contextual implications for semantic development (cf. §§ 14, 176-7, 401), are easily overlooked among the mass of data, and in this case the subject index gives no guidance, full and detailed though it is. While the final chapter of general conclusions goes some way towards remedying this difficulty, it is itself to a considerable extent a presentation of details, and includes a formidable tabulation of 'the distribution of direct means of intensification into different authors'. However, to draw attention to this aspect of the book is more a complaint than a criticism. Thesleff has made accessible and subjected to classification and theory a body of facts which should be of very great use, especially to linguists and to editors of texts, and has indicated lines along which further work in this field may proceed.

D. M. JONES.

#### ΠΟΙΗΜΑ. Ricerche sulla teoria dell'linguaggio poetico nell'Antichità. By A. ARDIZIONI. Pp. 129. Bari: Adriatica Editrice, 1953. L.1600.

This book is a study of the semantic development of the terms *ποίημα* and *ποίησις* from the fourth century B.C. onwards, and of the problems thereby raised in the history of literary criticism. It hinges mainly upon two divergent views held respectively by Philodemus and by Neoptolemus of Parium, Horace's principal source for his *Ars Poetica*; for the latter we are dependent upon the polemical statement (in a mutilated papyrus-text) of the former. Neoptolemus appears to have discussed poetry under three headings: *ποίημα*, *ποίησις*, and *ποιητής*, in the sense of poetic diction, poetic content, and personality of the poet. Philodemus deprecates the third as a parallel 'category', and claims that the normal and correct distinction between the first two is that *ποίημα* should mean a poetic 'work', *ἔργον*, whether complete or only a portion of a larger poetic complex, a few lines or a rhapsodic extract of an epic, while *ποίησις* is *ἔπη*, a tissue woven of many *ποίηματα*, even though such a *ποίησις* as the *Iliad* may itself also be a single *ποίημα*.

To us, both sets of definitions may well seem pedantic and unnatural, but the author reminds us that it is useless to pass judgement on ancient concepts and theories from the standpoint of modern criticism; our task is to study their historical development and note the occasional glimpses of a clearer vision of the nature of poetry. He assembles and examines all available evidence to show how these concepts could be derived from Plato and Aristotle through Theophrastus, Lucilius, Varro, etc., and how they are modified in Horace, Posidonius, and Dionysius of Halicarnassus, with a detailed account of the way in which Aristotle's two complementary criteria of *ποιητικὴ μέτρος*, choice of vocabulary and arrangement of words, developed into rival schools of thought. His command of the material is impressive, and the reasoning is clear and usually cogent in spite of its intricacy and the strain of following an argument that is divided so evenly between text and footnotes. Perhaps one danger of this concentration on historical development is the tendency to see echoes of grammarians' terminology even where the words have returned to their natural meaning. It is difficult to see that any trace survives in Horace of the mechanical, quantitative limitation of 'poema' (= short poem or limited part of a larger one) clearly visible in Lucilius and Varro, or that when he says *A.P.* 361 'ut pictura poesis' he means 'a large epic is like a picture' and not simply 'poetry is like painting'. 'Pictura' is indeed not seldom used in the sense of 'picture', but 'poesis' here is decisive for the abstract sense of both nouns; of course the thought 'a poem is like a picture'



is also implied in the context. Again, when Cicero says (*Or.* 20, 67) that the 'locutio' of Plato and Demosthenes is more of a 'poema' than comedy is, he is surely saying that their 'discourse'—we should say 'writing'—is like 'so much poetry' rather than using 'poema' in the sense of 'poetic language'; similarly, when Dion. Hal. contrasts *πρὸς λέξιν* with *πρὸς νόμον* and *μῆτος* it is not *πρὸς νόμον* used as 'diction' here, but *λέξιν* that has become concrete, 'prose-writing', in place of the more ambiguous *λόγος*.

A. M. DALE.

**ΔΑΙΜΟΝΙΕ.** An inquiry into a mode of apostrophe in old Greek literature. By E. BRUNSTEN-NILSSON. Pp. 155. Uppsala: Almqvist & Wiksell, 1955. Sw.Kr. 20.

This is a really serious and intelligent attempt to solve an old puzzle, viz. what exactly is implied by addressing a person or persons as *δαίμονι* (-οι, -οι, -ην). The authoress, who writes excellent English, finds a serious fault in earlier attempts at interpretation, namely (p. 10) that scholars have been too much inclined to start from the obvious derivation of the word from *δαίμων* and to let their views of the adjective's meaning be coloured by the connotations they attach to that substantive. Her own method is to start from those passages, in Homer, Hesiod, Herodotus, Aristophanes, Plato, and a few selected Alexandrians who use Epic language, in which the vocative occurs, and discover by an acute analysis what the tone given them thereby is, especially whether it is true that the adjective often implies reproach. The analyses are always interesting, showing good literary taste as well as wide reading and accurate scholarship. Thus in *Ω* 194 any such rendering as Pope's 'unhappy consort of a king distressed' is rejected, because 'that would be a bad way of trying to win her [Hekabe] over to his [Priam's] concept of what ought to be done. . . . The vocative is psychologically most closely connected with this imperative [in 197, *ἀλλ' ὄψεαι μοι τόδε στήν*]. In Eumaios' use of the word in *ξ* 443 she finds 'a polite formula for an invitation in an intimate situation' (p. 19). A fairly long list of supposedly reproachful uses (pp. 19-37) is treated with good insight into Homeric courtesies, and she concludes this part of the discussion by calling the word polyvalent and non-specialised, its function being 'to influence his conduct [i.e. that of the person addressed] in the direction desired by the speaker'. By way of contrast, she goes on to consider other forms of address, complimentary and the reverse, to be found in Homer, and notes that the rest of the sentence or speech regularly explains why someone is addressed, e.g., as *θεῖα*, whereas with *δαίμονι* we are left to gather the reason for its use as best we may. But at all events it is 'informal . . . impulsive . . . unsuitable in an official speech . . . incisive . . . does not appear to have any oratorical value' (p. 34).

Hesiod puts the address into the mouth of the reconciled Kottos addressing Zeus rather as comrade to comrade than as inferior to superior (*Theog.* 655; p. 65), while the hawk in *W.D.* 207 is 'cynical . . . pitiless . . . supercilious' (p. 67) and uses a 'formal and polite, non-evaluative' term to the unfortunate nightingale. Apollonios (pp. 74-5) makes Jason and Medea use the word to one another in an intimate conversation in which each is most anxious to impress the other, she with the urgency of her claims, he with his sincerity. Corresponding results are arrived at for Theokritos and Herodotus, while Aristophanes (p. 97) is found using the address analogously to if not identically with the Epic employment of it, this being part of his elaborate machinery for producing a comic effect. In Plato it regularly forms part of an address meant to arouse the interest of the interlocutor, often at some important point of the argument, 'particularly in the excursions and interludes inserted in the dialogue' (p. 108), and other places where the author 'wishes to stimulate his reader'.

The concluding pages, 115-44, form a valuable summary of the evidence set forth in the rest of the monograph. *δαίμονι* undoubtedly implies that the person addressed is somehow affected by or connected with a *δαίμων*; now how exactly is a *δαίμων* conceived? The answer, with which I am much disposed to agree, is that whereas a *θεός* no doubt possesses power, whether or not he is now using it, a *δαίμων* is thought to intervene only when it is plain that *numen* of some sort is actively present. Surely, if anyone is told that he is in such a 'numinous' condition, it is a good way of inducing him to action, positive or negative. As to translation into a modern language, that must be left to the taste and knowledge of the translator; there is no safe stock equivalent.

The printers have not done their work quite so well as Swedish compositors usually do, though gross misprints do not occur. The authoress has slipped once or twice: p. 65, it is not quite accurate to call the Hekatoncheires giants, though

they are gigantic in our sense; p. 69, no discussion of the language of Ap. Rhod. should be without a mention of that great source-book, Merkel's *Prolegomena* to his larger edition (Leipzig 1854); p. 93, I do not know why the heroine of *Ecl.* is called Praxagoras; p. 128, *ἔγχεος* is never 'sword' in Homer.

H. J. ROSE.

**La Notion du Divin depuis Homère jusqu'à Platon.** (*Entretiens sur l'antiquité classique, I.*) By H. J. ROSE, P. CHANTRAINE, B. SNELL, O. GIGON, H. D. F. KITTO, F. CHAPOUTHIER, and W. J. VERDENIUS. Pp. vii + 308. Vandoeuvres (Genève), Fondation Hardt, 1954 (Cambridge: Heffer). £2.

This book represents the first-fruits of an interesting venture. Baron Kurt Hardt, the owner of an eighteenth-century villa near Geneva, has established there a foundation for the study of classical antiquity with three main objects: first, the building up of a library at which scholars, especially young scholars, may as guests of the Foundation pursue their work for a period in peaceful and attractive surroundings and make the acquaintance of those from other countries who share their interests; second, the holding of annual *Entretiens*, at which a small group of scholars from different countries are invited to spend a week at the villa and read papers on aspects of a selected general theme: the papers are followed by a discussion at which a stenographer is present, and the whole is published in book form; third, Baron Hardt hopes also in the future to publish other works of classical scholarship under the imprint of the Foundation. His general aim is to contribute to the growth of international collaboration in the classical world, and provide a soil in which it may flourish.

The theme of the first *Entretien* was the conception of God from Homer to Plato, the second, now (August 1955) in proof, dealt with Greek influences on Roman poetry, and the third, which were held in August 1955, with some aspects of the Platonic tradition in pagan and Christian thought. For 1956, a symposium is planned on ancient historians. The names on the title-page of the first volume are sufficient guarantee that the Baron chooses his teams wisely, and the novel experiment of the recorded discussion works surprisingly well. I do not know how much the conversations of the first year have been re-touched for printing, but can vouch for it from recent experience that even in their original, spontaneous, and oral form they can be most stimulating and contain individual suggestions of permanent value. When possible, a paper is read in the morning and discussed about five o'clock, which gives time for a little reflection or even library-work on the part of those who are not tempted by the countryside or the lake, and another feature contributing to their success is the introduction of a slight, but by no means excessive, touch of formality, each of the team acting as chairman in turn.

I make no apology for explaining at some length the background of the *Entretiens*, which not only deserves to be more widely known, but should make it clear that the book that I am here reviewing really reviews itself. When each paper is followed by the comments of six other scholars with different points of view and different national traditions of scholarship (the countries represented were Switzerland, France, Germany, Holland, and England), there is not much of real importance still to be said. A brief indication of the contents will, however, be of interest.

In an introductory lecture marked by a praiseworthy caution and conservatism, H. J. Rose outlined the extent of our knowledge of the main tendencies in Greek religion, emphasising our ignorance of its origins (and hence of the causes of many puzzling features), and contrasting, in particular, Homeric religion and that of the less privileged classes, first represented for us by Hesiod. This provided especially useful background for the two following papers, those of Pierre Chantraine on Homer and Bruno Snell on Hesiod. Olaf Gigon contributed a closely-reasoned disquisition on the vexed questions of theological ideas in the Presocratic philosophers, in the course of which he made the interesting suggestion that if thinkers like Anaximander, Parmenides, or Anaxagoras did not apply the actual word *θεός* to their first principle (*ἀναρχος*, *θεός*, *νοῦς*), this was because 'das Wort *θεός* hat nicht genügend Rang, um das zu bedeuten, was mit *ἀναρχος* oder *θεός* gemeint wird'. After all, it would perhaps have been no compliment to the ultimate being, or the source of all being, to put it in the same class with Ares, or even with Zeus in some of his aspects. The neuter, the abstraction, may have suggested even higher divinity, a point which fitted well with certain remarks made by Chantraine and others in different connections. In *The Idea of God in Aeschylus and Sophocles* H. D. F. Kitto spoke in particular of the double plane of tragic action—human and divine—and explained the function of the divine plane as being to universalise the particular human



actions. 'The gods do not control the human action. . . . To borrow a term from mathematics, they are there as a system of co-ordinates. . . . As we watch the particular human actions against these co-ordinates, we can read their values.' F. Chapouthier saw three aspects in Euripides's attitude to the gods, not only the sceptical but also a strongly conservative side, delighting in ritual and liturgy, and thirdly, a positive side giving new content to the idea of God which his critical sense had purged. The last lecture, by W. J. Verdenius, on Plato's conception of God, goes deep and should be read and pondered (not necessarily agreed with) by all who hope for a better understanding of Plato. Taking as his main theme the relation between divinity and the Ideas, he brings them very close, even speaking of an 'Identität von Gott und Ideenwelt', by means of the formula 'je göttlicher, umso unpersönlicher'.

This last formula linked up with what, as Chapouthier rightly remarked in the ensuing discussion, had proved to be a leading motif throughout the meeting, from Rose's passing mention of *mana* and related concepts, and certain suggestions of Chantaine about the original conception of the Homeric gods as natural forces, through the remark of Gigon quoted above, to the final lecture of Verdenius: that is, the thought that 'la notion du divin, dans la conscience grecque, précède l'idée de dieu'. Historically, this may be difficult to verify, but logically it is true.

W. K. C. GUTHRIE.

**Geschichte der griechischen Religion.** By M. P. NILSSON. Pp. xxiii + 872, with 52 plates and 8 text figures. 2nd edition. [Handbuch der Altertumswissenschaft V.2.1.]. Munich: Beck, 1955. DM. 70.

The first edition of this standard work came out in 1941, and for obvious reasons did not reach this *Journal* then or for some years after. It has, however, so completely established itself ever since it became available in this country that the reviewer takes it as known and deals only with the alterations, comparatively few in number and small in extent, which have suggested themselves to the author in fourteen years. The book is almost exactly fifty pages longer than it was. Of these, eight are accounted for by the revision and partial rewriting of the introductory section, two (253-5) by the addition of a short account of the Muses, and three more (812-15) by a discussion of deified abstractions. The remaining thirty-seven are the result of a multitude of small corrections and additions, mostly in footnotes, which bring the information up to date almost everywhere (one or two quite trivial further changes are suggested in a notice shortly to appear in *Class. Rev.*), and include numerous brief criticisms of books and articles published since 1941. Some examples of the new material follow.

Concerning the Muses, N. rejects the suggestion that they were a kind of nymphs or especially connected with springs or water, is inclined to think that their Pieria was originally in Greece, not in Macedonia, and finds no certain etymology for their name, which is a very old one, apparently *urindogermanisch*. The discussion of deified abstractions is already so condensed that it seems unprofitable to try to epitomise it; it should be read and carefully considered as it stands. Another passage which says much in little space is the criticism of the late Father W. Schmidt's views, pp. 61 f.

A certain number of the new notes are rather subtractions than additions. Thus, p. 204, n. 8, disposes of the alleged discovery of the Delphic omphalos in the light of J. Bousquet's re-examination of the stone and its graffiti; and once or twice a monument of doubtful authenticity mentioned in the first edition has disappeared from the second. More positive additions to the work include such things as p. 444, n. 2, which gives a critical account of Schachermeyr's theories of the origin of Poseidon, while the text has been slightly altered to include Ventris's interpretation of the supposed dedication to that god from Asine, with a cross-reference to p. 343, n. 3, which sketches the same interpreter's 'sehr beachtenswerten Versuch' and especially his list of gods, from which (supposing his readings substantially correct) 'es folgt, dass Griechen schon während LM II in Knossos saßen'. The account of Asklepios is enlarged, p. 539, n. 7, not merely by mention of the work of the Edelsteins but by references to two full and critical reviews of it. Such improvements involve but little expenditure of ink and paper, but a great amount of critical reading, in this case several hundred pages to write 14½ lines of small print.

Here and there it might perhaps have been well to add yet another footnote or even make a slight alteration in the text. Thus, on p. 100, Rohde's theory that the Erinyes are originally vengeful ghosts is stated with approval. That is matter of opinion; but as other theories are mentioned in the notes, the one outlined (though not accepted) by Farnell, *Cults V*, pp. 437 ff., that they are hypostatised curses, was worth giving a

little space to. The reviewer personally accepts it as the most likely.

It should be mentioned that the printers have done their work very well. Their characters are everywhere clear and easy to read; the few misprints in the first edition have almost all been detected and put right, and the new ones are of the most trifling kind (e.g. φιδων for φιδου, p. 650, n. 2, line 5 from end; *impure* for *impur*, p. 848, additional note to p. 90), such as will mislead no one. A laudable feature is that the pagination of the first edition is given in square brackets on the margins of the present issue.

H. J. ROSE.

**De Téléphore au 'moine bourru'. Dieux, génies et démons encapuchonnés.** [Collection Latomus XXI]. By W. DEONNA. Pp. 167. Brussels: Latomus, 1955. B.Fr. 250.

A hood, whether or not attached to a cloak, was one of the commonest garments in antiquity, as indeed it still is. Consequently, there are many hundred representations in ancient art of all manner of persons, human, divine, mythological, or purely fanciful, wearing such clothing. The ingenious author sets himself to collect, classify, and interpret these monuments, with the result that, whatever one may think of his explanations, at least his material is useful and abundant.

He begins by explaining what a *cuclullus* was, where it was worn, and by whom. His reasonable conclusion is that it is not exclusively a northern (Gaulish) garment, for it is also found in Egypt and elsewhere; and indeed there seems to be no reason why several peoples should not have hit independently on so simple and obvious a device for protecting head and shoulders. On p. 24 begins the account of the significance of the garment, other than purely practical.

Firstly, it can signify separation, difference from the surrounding world, hence especially supernatural or at least sacerdotal character. It was worn at night, hence is appropriate to beings of the darkness; it has especially a chthonian character (p. 20; D., in my opinion, is given to over-emphasising this interpretation throughout the whole monograph, but that is not to say that there is no truth in it). It is especially appropriate to mourning and funerary rites, separating mourners, the dying, and the dead from the world of the living. Further, the *cuclullus* covers a number of grotesque phallic figures, a phenomenon whose interpretation (several are given on pp. 32-5) is doubtful.

With p. 37 we reach the supernatural figures who are thus hooded. D. begins with Telephoros, the child-godling associated (generally but not always) with Asklepios. Next (pp. 58 ff.) comes a Celtic figure, the so-called *genius cucullatus*, who seems sometimes (p. 66) to have become identified with Telephoros but probably is of independent origin. A number of deities occasionally wear the hood (Mercurius, Epona, Nehalennia, Harpokrates, Priapos, the Gaelic Caillcach Bheara). Now come (pp. 80 ff.) a number of personages who seem to have little in common except that they wear a hooded garment which is or might be drawn forward to cover the head. D. attempts, not always plausibly, to explain them. They are occasionally actors in costume; not a few may have some religious or magical significance, but I would class far more than D. does as flights of grotesque fancy, not always decent. In modern pottery there are plenty of curious little figures meant simply to be comical, together with others which are intended as mascots, Kewpie dolls side by side with Billikins. I do not see why it need have been otherwise in antiquity, especially as we hear of many comic statues and pictures and still have some survivals of this genre in our museums. Because ancient religion penetrated daily life to an extent strange to us, it does not follow that every oddity to be found in Greek, Roman, or provincial houses was intended to be worshipped, or to avert the Evil Eye.

After an account (pp. 115 ff.) of *cuclulli* associated with death and the after-life, a few pages (149-53) are devoted to an attempt to extract the general character of all these representations. Then comes a short final section (pp. 154-62) on survivals, which include the *moine bourru* of the title, a nursery bogey.

Misprints and loose references do not add to the value of the work.

H. J. ROSE.

**Deux études de symbolisme religieux: La légende de Pero et de Micon et l'allaitement symbolique. L'aigle et le bijou: à propos du collier d'Harmonie décrit par Nonnos** [Collection Latomus XVIII]. By W. DEONNA. Pp. 123, with 25 text figures. Brussels: Latomus, 1955. 175 fr.

The two studies are, firstly, 'la légende de Pero et de Micon', one of the many pairs of names for the good daughter



and the old father whom she kept from dying of starvation by suckling him. The story is traced into several classical and other authors and not a few countries, a useful assemblage of material, welcome to any folklorist. A more doubtful service to science is the attempt (pp. 15 ff.) to connect it with the well-known rite of adoption by real or pretended suckling, a procedure known to classical antiquity, as the episode of Hera and Herakles abundantly proves. To me it seems to have a totally different atmosphere, especially as it is a real performance with a definite end in view, not a folk-tale. Still, once more the collection of material may be found useful.

The author then goes on to examine 'l'aigle et le bijou', i.e. the necklace of Harmonia as described by Nonnos, *Dion.* v. 136 ff. with his usual verbosity. Neglecting many details, some rather obscure, we get a necklace in the form of a two-headed snake, with a (double?) eagle serving as a cover for the fastening and spreading its wings (and legs?) over the concealed clasp, 158-60. D. has no difficulty in producing abundant examples of eagles or other raptorial, often in conjunction with serpents, especially the Egyptian uraeus, which have a religious significance, or at least a kind of heraldic one. On the strength of this he concludes (p. 120), 'On voit combien sont variés et complexes les notions et les prototypes figurés que Nonnos a utilisés' to construct his imaginary ornament, which in earlier authors is a far simpler thing when it is described at all. Once more I feel very doubtful. Nonnos is full of learning of a kind, but it is almost exclusively literary and mythological, with a thin layer of astrological knowledge overspreading it. I doubt if he knew or cared much about the symbolic meaning of religious emblems, Egyptian or other. Yet again, the collection of material may well prove of use to other researchers, but the conclusion drawn remains extremely hypothetical.

There are a few minor slips of author or printer. On p. 6 Valerius Maximus, when he wrote *Perus* (v. 4, ext. 1), was using a genitive case. On p. 7 the name of Hyginus should be moved down one line. P. 52, n. 1, I wrote only a part of the introduction and notes of the Loeb Nonnos. P. 117, I think it excessively unlikely that Nonnos composed the *Dionysiaca* after turning Christian.

H. J. ROSE.

**A History of the Crusades.** By S. RUNCIMAN. Vol. I. *The First Crusade and the Foundation of the Kingdom of Jerusalem.* Pp. xiv + 377 + 8 pl. + 5 maps. C.U.P., 1951. 25s. Vol. II. *The Kingdom of Jerusalem and the Frankish East.* Pp. xii + 491 + 8 pl. + 6 maps. C.U.P., 1952. 42s. Vol. III. *The Kingdom of Acre and the Latter Crusades.* Pp. xii + 530 + 15 pl. + 5 maps. C.U.P., 1954. 35s.

The international team under American editorship has so far only produced the first of its five volumes on the crusades, but meanwhile Mr. Runciman has already completed his own highly individual account of this movement, and has given us what is, within his self-imposed limits, the best full account in English. He views the crusades primarily as the struggle with Islam in the East Mediterranean, and is not concerned with other areas, as Spain or the Baltic. Like C. D. J. Brandt (whose recent *Kruisvaarders naar Jerusalem* is not mentioned in the bibliography), he begins with the Heraclians and Mohammed and gives a survey of Byzantine activities on the eastern frontiers down to the eleventh century. He then all too briefly traces the growing interest of the Latin West in the holy war against Islam (already admirably investigated in the work of G. Erdmann) and analyses the various reasons for the First Crusade. Volume I closes with the establishment of virtually independent Latin principalities in Syria and Palestine, and the coronation of Baldwin of Edessa as King of Jerusalem in 1100. The skilful narrative has already laid bare the intricacies and hazards of the international situation: the Byzantine Emperor's policy of consolidating his position in Asia Minor before proceeding farther afield, and Latin reluctance to accord him full recognition as overlord of the lands regained from the infidel; Norman ambitions, fully revealed in the plans and attitude of the remarkable Bohemund I of Antioch; the conflicting cross-currents of the various native Christian communities long established in the reconquered lands and the guarded hostility of the neighbouring Armenian kingdoms recently set up in the Taurus and Anti-Taurus regions; and—perhaps most important of all—the disunity within the Muslim world which more than anything else made possible the temporary successes of the Christians.

All these threads are admirably followed up in Volume II, which describes the precarious fortunes of the Latin Orient in the twelfth century up to Saladin's capture of Jerusalem in 1187. The high-water mark of crusading achievement was the successful consolidation of the kingdom of Jerusalem under a secular ruler and not as a theocracy under direct papal control.

Meanwhile in the north an uneasy equilibrium was maintained: Antioch, Tripoli, and Edessa acknowledged the overlordship of Jerusalem; the Muslim princes were at loggerheads; and Constantinople was occupied with the Seljuks of Rum, who could not be dislodged from Asia Minor. Then, in the thirties, coming ill cast their shadow. There were already problems of succession in the crusading states. And Zengi, the atabeg of Mosul, was daily growing in power. His death in 1146 brought no respite, for still greater danger threatened from his son, Nur ed-Din. The Second Crusade of 1147, led by the kings of France and Germany, was an abysmal failure, partly because its leaders, ignoring the advice of those who knew better, insisted on attacking Damascus instead of Aleppo. Nur ed-Din went from strength to strength, and the death-knell of the crusading kingdoms sounded when Shirkur and his nephew Saladin gained Egypt for him in 1169.

Within this framework of almost unceasing warfare and diplomacy in the Latin Orient, certain factors emerge, particularly the overriding influence of economic considerations and the mutual desire to avoid any action that was likely to upset the lucrative flow of trade which went through certain key ports, as Alexandria or Tyre; and also the constant clash between the Byzantines and the Latin Christians due to conflicting policies, embittered on the Byzantine side by the knowledge that as early as the beginning of the twelfth century the Norman Bohemund I of Antioch had urged Pope Pascal II to preach a crusade against Constantinople itself. The rising star in the Muslim world was now Saladin, and his victory over the crusaders at the Horns of Hattin and his capture of Jerusalem (1187) meant the permanent disruption of the Latin principalities. As Volume III shows, all that local effort and the Third Crusade could salvage from the wreck was a small strip of coast including Acre and Tyre and the access to the Holy Places, and this was largely due to the good generalship of Richard I. In the north the shrunken remnants of the principality of Antioch still remained. At this juncture the West failed to follow up the Third Crusade and take advantage of Saladin's death in 1193 and the subsequent disunity in the Ayyūbīte family. Unfortunately the Fourth Crusade was diverted, first to Zara and then to Constantinople. The capture and sack of Constantinople and the partial conquest of the Byzantine Empire followed, with disastrous results for the Christian settlements in Syria. It meant a hardening of the rift between Eastern and Western Christendom, and it diverted western crusaders from Palestine and Syria to the wealthier lands of the Aegean. Crusading history after 1204 is of necessity in the nature of an epilogue: Cyprus, the Kingdom of Acre, and the minute principality of Antioch were isolated in a Muslim world. Neither the Emperor Frederick II, nor St. Louis of France, nor the diversion created by the advance of the Mongols, nor Edward of England could stem the tide. And in the fourteenth and fifteenth centuries the danger had shifted: it was now a question of keeping the Turks out of Europe itself. The melancholy failures of Nicopolis and Varna were inevitably followed by the fall of Constantinople to the Ottomans in 1453.

Thus even the briefest survey of crusading history during this period of over 400 years reveals its complexity. Mr. Runciman has handled his difficult, and often contradictory, material admirably. Without oversimplifying and with easy mastery he moves from Antioch to Jerusalem, from western papal circles to imperial Byzantium or Muslim emirates. The clarity of his presentation and the vigour of his literary style carry us swiftly through his three volumes, while personal knowledge of the countryside and admirable maps and illustrations add to our understanding and enjoyment. The outstanding characteristic of this history is the attempt to do justice to the Byzantine Empire, emphasising the extent to which it contributed to Christian successes in the twelfth century and pointing out that in the past it has too often been made the scapegoat for failures due to Latin dissensions and inadequacies. Bohemund I of Antioch, in particular, is one of the main villains of the piece for Mr. Runciman, who attributes to his misrepresentations the early poisoning of the west and the papacy against Constantinople. It is indeed in dealing with eastern circles that Mr. Runciman is most at home. He appreciates the rôle of the native Christian element in the East Mediterranean, for instance, in the Armenian principalities or in Edessa and the other Latin principalities, distinguishing between the Orthodox and the separated churches, and bringing out the important part often played by them. Such Latin sympathies as he has are for those westerners who were sensible enough to come to terms with the East, as Baldwin I of Jerusalem. His delineation of character is admirable, both of those whom he praises, as Saladin or John II Comnenus, and of those who come in for his trenchant criticism.

In his introduction to his third volume Mr. Runciman disarmingly states that 'an author must write his book in his own way'; and he has already made it clear that he has modelled



himself on the old chroniclers 'who knew their business' and whose main concern was warfare. For a historian to define his own terms of reference is fair enough, but in what claims to be 'a history of the crusades' it may well be objected that there is all too little on social and constitutional, economic and cultural, developments. Volume II has only a short chapter on 'Life in Outremer', and Volume III gives one chapter on 'The Commerce of Outremer' and one on 'Architecture and the Arts in Outremer'. Rather more on the ecclesiastical side might also have been expected, as this is so vital an aspect of relations between East and West with which the author is especially concerned. Here there are omissions, as for instance, Manuel Comnenus' negotiations with Eugenius III (*cf.* V. Grumel, *Études Byzantines* III (1945), 143 ff.). There seems to be some confusion between the official schism of 1054, marked by the exclusion of the Pope's name from the diptychs, and the hardening of the rift into open antagonism in the course of the twelfth century (*cf.* A. Michel's writings on the subject). Thus the history of the crusading kingdoms, as distinct from politics, diplomacy, and wars, has yet to be written. A good deal of work has already been done on those aspects about which Mr. Runciman has little to say, and the lines on which such a history might be written have already been indicated by R. C. Smail, in his stimulating article 'Crusaders' Castles of the Twelfth Century' in the *Cambridge Historical Journal* X (1951) and elsewhere.

In a work of this kind the bibliography cannot be foolproof, but there are some unaccountable omissions, for instance, some of the best recent work on castles is missing, as C. N. Johns on 'Atlit'. Recent translations of the crusading historians might well have been cited more often, and the best editions are not always given. The system of not repeating the bibliography of Volume I in Volume II, and so on, is unfortunate, as it means that it is often essential to use all three volumes together. The method of citation in the footnotes does not make for clarity, and sometimes the reader would have been helped by a brief reference to Dölger's *Regesten*, or to Ostrogorsky, *Geschichte des byzantinischen Staates* (where full and excellent bibliography and comment are provided). Often the year of a periodical cited is omitted, and there are some misprints.

But, in spite of sweeping statements in the grand style, overpartiality for politics and wars, faults of omission and minor inaccuracies, this history can stand with Grousset and the earlier German works. It gives a most stimulating survey, and its outstanding merit is that in at least one respect it attempts to suggest to English readers a long-needed re-orientation of outlook.

J. M. HUSSEY.

**Dumbarton Oaks Papers, Number Eight.** Pp. 330, with 42 illustrations. Cambridge: Harvard University Press, 1954 (London: Geoffrey Cumberlege). 60s.

This number of *DOP* contains ten articles, besides a short biography and a bibliography of the late Robert Pierpont Blake, to whose memory the volume is dedicated. Professor Alphonse Dain, in an article called *La transmission des textes littéraires classiques de Photius à Constantin Porphyrogénète* (pp. 31-47), studies the translation into minuscule and the editing of classical texts from the middle of the ninth to the end of the tenth centuries. The study is rightly made against its historical background. The secular university of Bardas Caesar (856-866) and of Constantine VII (944-959) promoted the centralisation in Constantinople of classical texts and their reproduction. First came philosophical and rhetorical works, next historical, and lastly, in the second half of the tenth century, poetical. (This is no doubt the evidence of surviving MSS.; but we must add that we have excellent evidence that the poets, especially Euripides, were studied by a section of society at least as early as the beginning of the tenth century.) The article is also valuable in bringing to light the preparatory work done in the collection of material even before Photius (who became patriarch in 858, not 857) by such scholars as John the Grammarian (who was deposed in 843, not 844).

In a long, admirably documented, and absorbingly interesting study (*The Cult of Images before Iconoclasm*, pp. 85-150), Professor Ernst Kitzinger traces the intensification of the superstitious reverence paid to wonder-working icons between the middle of the sixth century and the iconoclastic reaction in the second quarter of the eighth. Of the fact, no one who has read K.'s article can entertain any doubt. The reasons are, as he admits, not so clear. The doctrinal importance, underlined by the Trullan Synod, of illustrating the Incarnation through anthropomorphic representations of Jesus Christ; the deliberate policy of the secular rulers of a collapsing world empire in shifting emphasis from an universal prince to the 'Only Ruler of Princes': these appear merely as motives for the official

encouragement of a tendency essentially and profoundly popular. The whole history of the Iconoclastic movement is an object lesson in how little can be done, even by the most absolute authority, to change the instinctive beliefs of a populace. And the general tendency towards obscurantism from the later sixth century onwards may be regarded, in the broader view, as but one more symptom of the death of the Ancient World, a phenomenon which led Bacon to describe Justinian I as the last of the Romans, and Ostrogorsky to see in the Heraclian age the beginning of Byzantine history properly so denominated.

Professor S. Der Nersessian publishes (pp. 203-24) an interesting Armenian version of the 'Harrowing of Hell', from a fourteenth-century MS. in the Armenian Patriarchate of Jerusalem. The version shows many points of similarity with Greek homilies on the same subject ascribed to Eusebius (? of Alexandria; ? sixth century), but in their surviving form probably not older than the eighth century. The Armenian version may rest on an earlier Greek tradition. The author promises publication of other Armenian homiletic texts, hitherto equally unknown.

Notable among other contributions are the publication of an elegant liturgical Roll of the eleventh century, now in the Greek Patriarchate of Jerusalem, by Professor André Grabar; and a very lucid account of the Latin Patriarchate of Constantinople (1204-61), together with some hitherto unpublished letters of Pope Honorius III, by Professor Robert Lee Wolff.

R. J. H. JENKINS.

**The Madaba Mosaic Map, with introduction and commentary.** By M. AVI-YONAH. Pp. 80, with 10 + 2 plates and 14 text figures. Jerusalem: Israel Exploration Society, 1954. Price not stated.

In this attractive volume Professor Avi-Yonah, Lecturer in Byzantine Archaeology at the Hebrew University in Jerusalem, offers a new study of the famous map mosaic discovered in 1884 at Madaba (Jordan). The study, which is illustrated with half-size reproductions of the coloured lithographs of the mosaic published in 1906 by Palmer and Guthe, had appeared previously in Hebrew, both in book form and as an article in Volume II of *Eretz-Israel* (1953).

The author gives a detailed description of the extant portions of the map, section by section, with a transcription and explanation of all the legends which appear thereon. In his introduction he deals with the question of the original extent of the mosaic; its place in the church whose floor decoration it formed; its date and technique; the peculiarities it presents from the cartographic point of view; the content, purpose, and significance of the map; and the sources on which it is based.

It had long been noted that a majority of those legends on the map which refer to biblical sites in Palestine depend on the *Onomasticon* of Eusebius of Caesarea. Avi-Yonah shows that there was in addition a cartographic source in the form of a Roman road map of the kind known to us through the Peutinger Table. He concludes that such a map, augmented by data gleaned from Eusebius and by additional features which became prominent in Palestinian topography after the time of Eusebius, served the mosaicist as a model and was adapted by him to the available floor space.

The ground plan of the church to which the mosaic belonged has not been ascertained with any precision. To accommodate the known fragments of floor decoration Avi-Yonah proposes an ingenious reconstruction in which the map occupies a kind of transept and does not extend over the whole area of the nave. From the architectural point of view, however, the proposed ground plan is unusual, to say the least (p. 14, fig. 3).

The question of the architectural setting of the map obviously bears on its shape and extent. This, in turn, is intimately connected with the problem of its content. According to Avi-Yonah, the map in its original form covered 'biblical Palestine and parts of the adjacent lands connected with the Bible' (p. 15). It is true that the extant parts of the map refer largely to Palestine and that there is an obvious, though not exclusive, emphasis on biblical sites. The inclusion of parts of Egypt is also compatible with Avi-Yonah's interpretation. But a number of early eye-witnesses claim to have seen on the mosaic—which suffered badly during the years following its first discovery—the names of places far away from Palestine, and the question remains whether originally it did not cover, if only perhaps in a much foreshortened rendering, a good deal more of the *oikumene*.

Whether or not biblical topography occupied as large a part in the whole as it does in the extant fragments, the author's

<sup>1</sup> Ch. Clermont-Ganneau, *Recueil d'archéologie orientale*, II, Paris, 1898, p. 174. A. Musil, *Arabia Petraea*, I, Vienna, 1907, p. 116.



contention that the purpose of the map was simply didactic is open to question. The Madaba mosaic—a work of the advanced sixth century, as Avi-Yonah rightly points out—must be considered in conjunction with other church floors of that period in Palestine and elsewhere on which encyclopaedic and particularly geographic and cosmographic subjects are prominently displayed.<sup>2</sup> Though more elaborate than any other extant example of this 'scientific' genre the map mosaic represents a widespread trend to make use of the floor in order to bring aspects of the physical universe into the confines of the church. The primary intent in doing so was almost certainly symbolic rather than didactic. Indeed, it was during this same period that the church building itself came to be interpreted as a symbol of the universe, an interpretation which could hardly find a more appropriate and palpable expression than through the rendering of the floor as a map.

The fragmentary inscription quoted on p. 16 and referred to again on p. 18 was not found in this church, but is part of a longer inscription found in the church of the Theotokos.<sup>3</sup> The illustrated article on Madaba by Pavlovsky,<sup>4</sup> based on materials collected by Kluge, is important and should not be omitted even from a selective bibliography. Kluge, incidentally, was the 'unknown Russian visitor' whose drawing is referred to in Avi-Yonah's preface (p. 7). The drawing was in the possession of the Palestinian Orthodox Society when it was photographed for the *Ecole des Hautes Etudes*.<sup>5</sup>

ERNST KITZINGER.

**The Oldest Slavonic Tradition of Byzantine Music** (*Proc. Brit. Acad. XXXIX*). By C. HÖEG. Pp. 30, with 4 plates. London: Geoffrey Cumberlege, 1954. 7s. 6d.

Those who, unlike myself, had the privilege of hearing this paper read before the British Academy, must have been amazed at the speaker's learning and appalled by the difficulty and complication of the subject. It is therefore a good thing that Professor Höeg's lecture has come out in booklet form. By the aid of photographs, diagrams, and musical illustrations he has packed into a narrow space an almost incredible amount of information, most of it entirely new to English readers. Hitherto the few extant MSS. of early Russian neumes have been so jealously guarded (especially in the Synodal Library at Moscow) that hardly any foreign scholars had ever seen them, but were dependent on the poor facsimiles given by Metallov and other Russian writers. Professor Höeg seems to draw upon an ample store of photographs; and he has achieved a diplomatic triumph in obtaining them. Russian musicians have worked diligently on their traditional hymnody; but they have not deciphered any neumes older than the Hooked Notation (*Kryuki* or Early Sematic) which flourished in the sixteenth and seventeenth centuries. This is unlike any Byzantine system; but it has been fully expounded and may be learned from O. v. Riesemann, *d. Notationen d. altruss. Kirchengesangs* (Höeg reproduces a specimen, Pl. I). In the late seventeenth century it was largely superseded by the Hammer-headed Notation on five lines (*ibid.* Fig. 1, p. 41)—a modified staff-method, borrowed from the West. Russian music remained diatonic and escaped the Arabo-Turkish influences that were so strong in eighteenth- and nineteenth-century Greek church singing. After the seventeenth century the use of vocal harmonies made Russian music still less Byzantine; and visitors who admired the booming bass voices in St. Isaac's Cathedral and the rich, though austere, harmonies in the anthems of Kalinikov, would hardly reflect that the early Russian Christians not only translated the Greek hymns, syllable for syllable, but also took over the Byzantine melodies and the eight-mode system. Here Professor Höeg comes forward with a brilliant new theory. It has hitherto been held that the archaic or Kondakarial Russian neumes were the survival of an extinct Byzantine notation, of which only faint traces remained in the famous Chartres fragment. But Höeg declares the Kondakarial neumes to be a Russian invention, specially adapted to the florid *contacia* (preludes to the narrative

odes of St. Romanus and his school), and to have been made up from Byzantine signs, not previously combined, and perhaps used with different meanings. Many hairs will turn grey before a final verdict can be given on so abstruse a case. Yet Höeg has little hope of finding the key to this species of neume, although the Early Sematic Russian Notation, borrowed from the Greek Coislin System (early twelfth century), may perhaps be decipherable.

H. J. W. TILLYARD.

**The Classical Association. Jubilee Addresses: Are our pearls real?** By GILBERT MURRAY; **The Classical Association—the first fifty years.** By L. J. D. RICHARDSON. Pp. 35. London: John Murray, 1954. 2s. 6d.

This is a reprint of Gilbert Murray's presidential address and Professor Richardson's history of the first fifty years of the Classical Association. It would be impertinent to say anything about the President's address except that, as always, he restores courage and confidence to those who may sometimes doubt not whether their pearls are real but whether the amount of labour required to produce a remote and inaccurate image of a pearl is justified. Perhaps it may also be said that his own supreme craftsmanship as a writer adds a great deal of weight to his criticism of modern poetry, in which he sees the beginning of a change for the better.

The Classical Association was formed in 1903 'to impress upon public opinion the claim of classical studies to an eminent place in the national scheme of education'. It now has twenty-eight branches in this country, and is linked with eight Classical Associations overseas. Professor Richardson has written an eloquent and informative account of the history of the Association, which, besides pursuing its more formal aims, tries 'to create opportunities for friendly intercourse among all lovers of classical learning in this country'.

These two addresses are complementary: a statement of faith and an account of practice. The Classical Association can derive inspiration for its future from both.

T. B. L. W.

**Numen. International Review for the History of Religions. Vol. 1, fasc. 1 (January 1954).** Pp. 90. Leiden: Brill. Price not stated.

This new periodical is described as an 'International Review of the History of Religions' and is issued by the International Association for the History of Religions. The editor is Professor Pettazzoni, and this should ensure that a high standard of scholarship is maintained. This first number is almost entirely devoted to an article on Iranian religion by Professor Widengren entitled *Stand und Aufgaben der iranischen Religionsgeschichte*. It seems a pity that so much space in the first number should have been accorded to an analysis of the present state of specialised and semi-specialised research into Iranian religions, since, with all due respect, the importance of these religions has been exaggerated, and much that has recently been written about them is pure speculation. Moreover, Widengren's article adds little to what he has already written on this subject. A more varied contents in this initial number would have been welcome.

R. C. ZAEHNER.

**Festschriften.** By D. ROUNDS and S. DOW. (Harvard Library Bulletin, VIII, 3, 1954, pp. 283-298).

Articles are not customarily reviewed in these pages; but Miss Rounds and Professor Dow provide such useful information and comment on so important a subject that a brief notice is justified. After a summary, illustrated by tables, of the extent of the practice of presenting *Festschriften* to scholars in the Graeco-Roman field, the authors pronounce against the publication of separate volumes of *Festschriften*, on the scores of limited circulation and miscellaneity. They are rather more favourable to the presentation of individual numbers of serial publications. We record without comment the trenchant dictum of S. Griswold Morley (p. 285): 'The friends of a faculty man distinguished chiefly for his longevity and good nature had better lay in his lap some other gift than a tome browbeaten from a chilly world of erudites. Let them give him a watch and chain, a jewelled sword, or a cheque for a thousand dollars.'

R. J. H. J.

<sup>2</sup> *Actes du VI<sup>e</sup> Congrès International d'Etudes Byzantines* (Paris, 1948), II, Paris, 1951, pp. 209 ff.

<sup>3</sup> *Revue Biblique*, I, 1892, pp. 641 ff.

<sup>4</sup> *Bulletin de l'Institut Archéologique Russe à Constantinople*, VIII, 1903, pp. 79 ff.

<sup>5</sup> G. Millet, *La Collection Chrétienne et Byzantine des Hautes Etudes*, Paris, 1903, p. 12.



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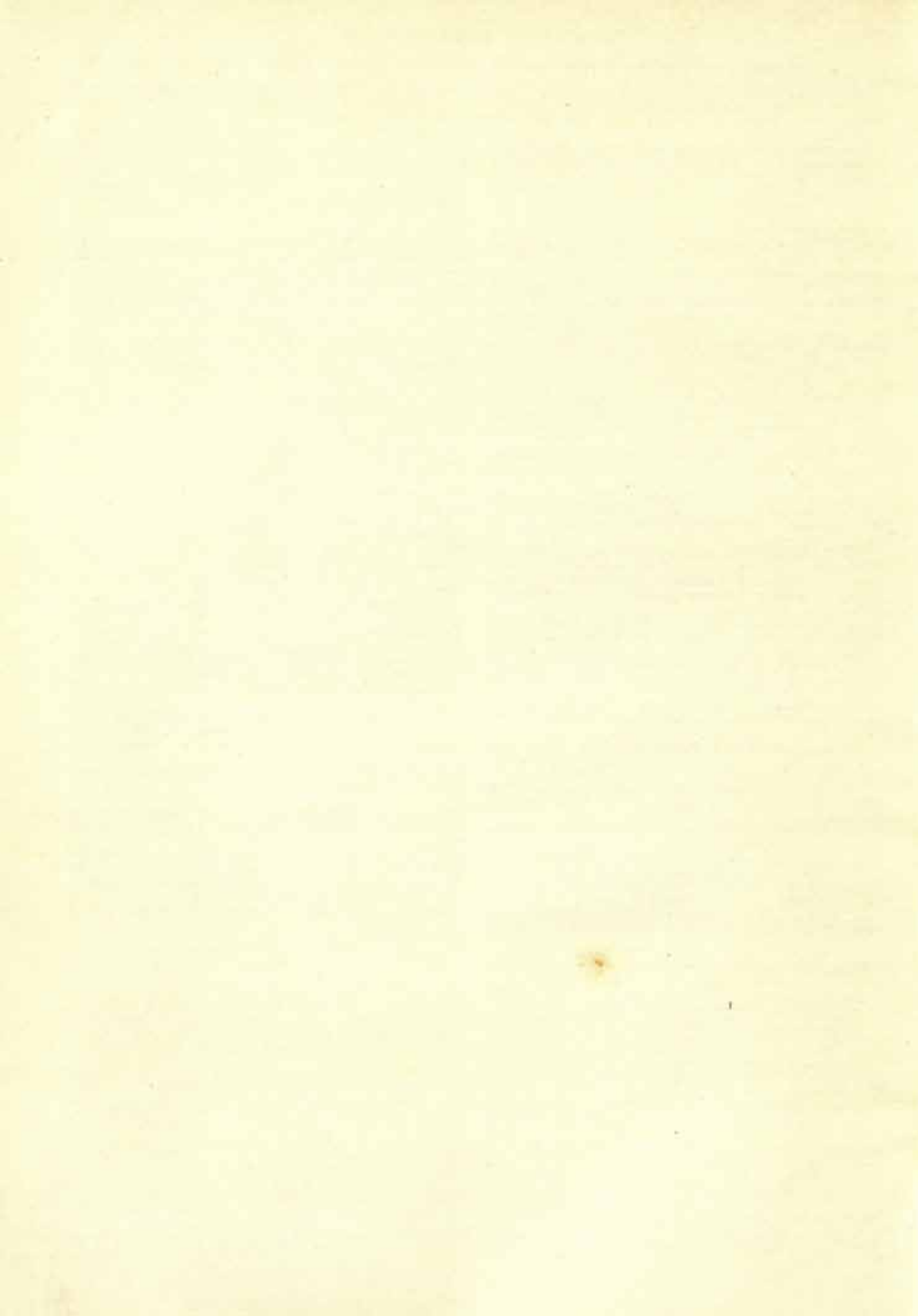
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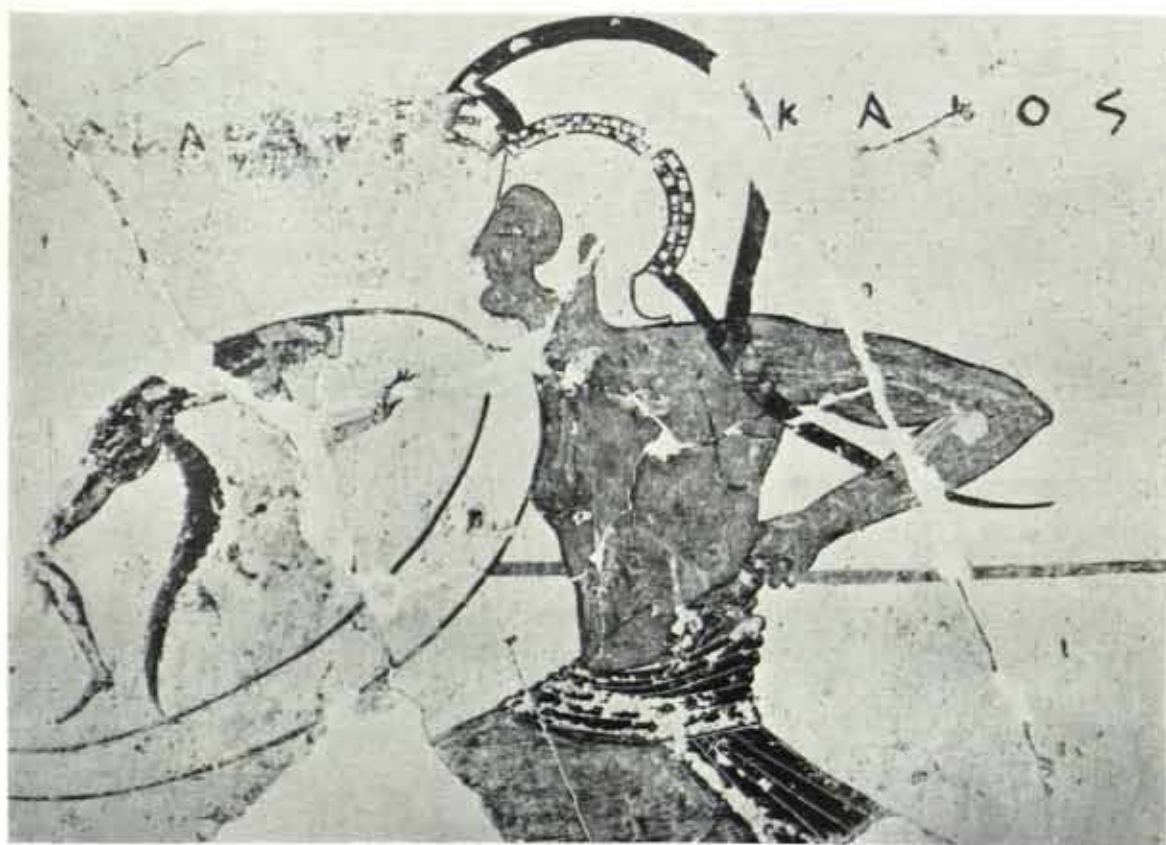


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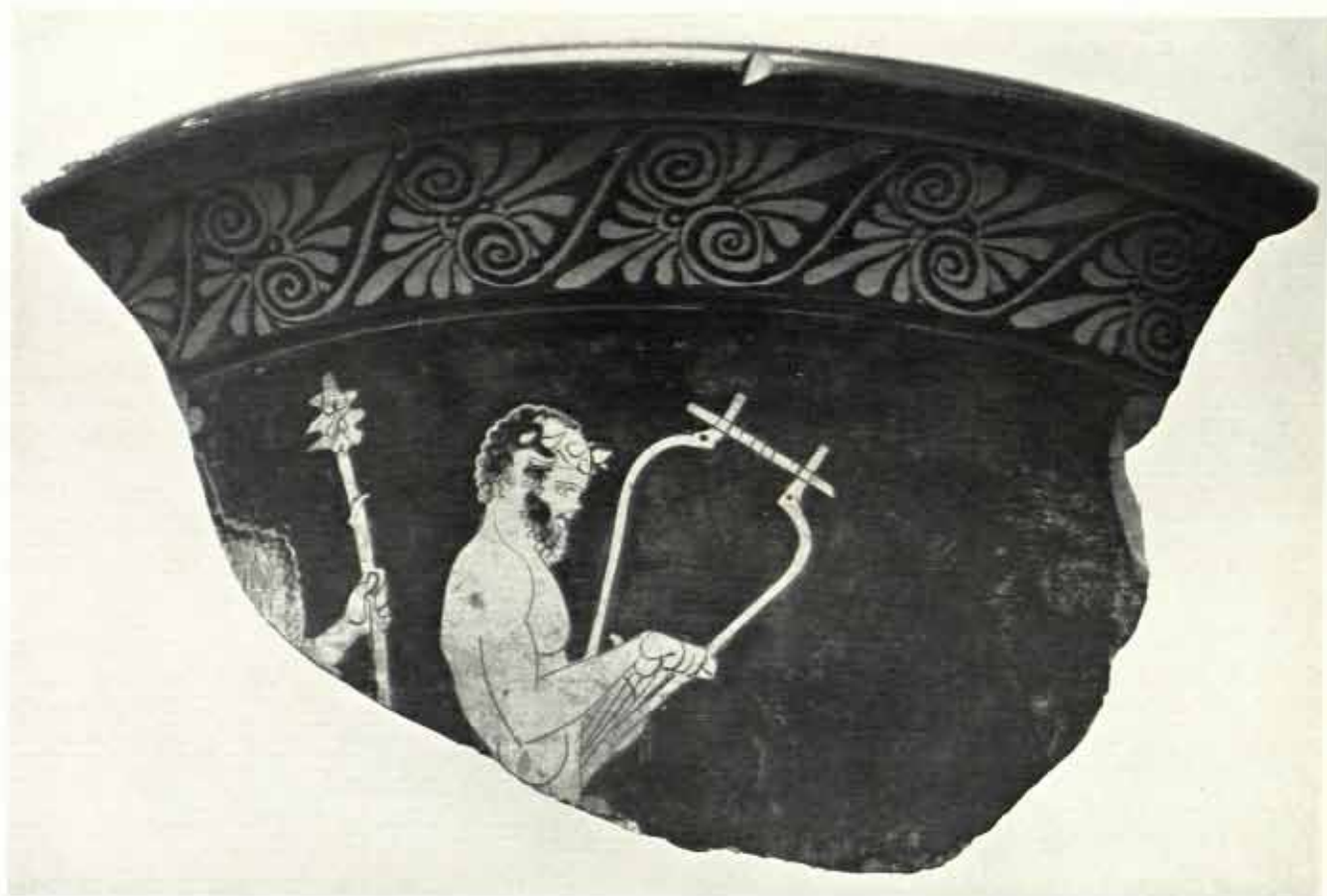


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2. Athens Akr. 1037.





1. Copenhagen, Thorwaldsen Museum, 97.



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# THE SOCIETY FOR THE PROMOTION OF HELLENIC STUDIES

50 BEDFORD SQUARE, LONDON, W.C.1.

## REPORT FOR THE SESSION 1955-56.

THE Council beg to submit their report for the session:—

There has been one important development in connection with 50 Bedford Square.

In view of the delay in providing accommodation for the Societies in the new building in Gordon Square, the University has generously agreed to assume full financial responsibility in connection with the tenancy of 50 Bedford Square, including the responsibility for carrying out repairs and improvements. In return, the Societies have agreed, when they leave 50 Bedford Square, to forgo any profit which might accrue from the disposal of the remainder of their agreement with the Bedford Estate. Under this arrangement the University has refunded to the Societies the cost of redecorations and improvements carried out since November 1954.

This relieves the Societies of some financial anxiety, but it has been decided to maintain in being the Sinking Fund, which has been accumulated for the purpose of meeting any unforeseen expenses.

Work began on the new building in Gordon Square before the end of 1955, and is fairly well up to schedule. The completion date is still November 1957, and it is hoped that the premises will be ready early in 1958.

### Finance.

The year's accounts showed a surplus of £335, as compared with £890 in the preceding year.

This decrease is partly accounted for by the fact that £250 has been set aside for a publications fund to enable the Society to publish occasional papers which, by reason of their length, are unsuitable for the *Journal*. There are also the increased costs of printing and maintenance. The Institute of Classical Studies is now making a fixed charge for maintenance based on an analysis of six months' costs.

The sales of the *Journal*, including back

numbers and other publications (£597), continue to make a substantial contribution to the Society's income, but this cannot be expected to continue indefinitely.

With the rise in salaries and costs, and with the production of an extra volume of the *Journal* (The Ross Volume), little or no surplus can be expected in 1956, and a small deficit is more likely. The Council has therefore decided to recommend to the Annual General Meeting that Rule 26 be amended, and that all members, irrespective of the date of election, should be liable to pay the full subscription rate of £2.

Recoveries under deeds of covenant amounted to £209 for 1955.

Membership figures as at December 31st, 1955, are shown below, with comparable figures for past years:—

	Members.	Life Members.	Student Associates.	Libraries.	Total.
1939	1,003	141	222	325	1,699
1951	991	123	174	376	1,664
1952	1,014	123	197	376	1,710
1953	1,004	124	165	385	1,678
1954	1,023	124	147	402	1,696
1955	1,087	125	139	426	1,777

### Obituary.

The Council record with great regret the deaths of Mrs. E. B. Culley, Miss Helen Farquhar, Mr. G. M. Gillespie, Professor S. R. K. Glanville, Mr. C. S. Gulbenkian, Miss N. M. Holley, Sir Douglas Orme Malcolm, Dr. Lauri Tudcer; and also of two honorary members, Fr. W. Freiherr von Bissing and Professor Ludwig Curtius.

Mrs. Culley, for many years, gave valuable voluntary help to the Library, to which she has bequeathed her collection of classical books.

### Journal of Hellenic Studies.

Members are reminded that, in addition to the normal *Journal* of 1956, there will be a special fascicule of approximately 130 pages in honour of Sir David Ross. This will, it is hoped, be published in December this year or January



1957, and will form Fascicule (i) of Volume 77 (1957). In response to an appeal, £295 has been contributed towards the additional cost by members and others.

In addition, the Society has been fortunate to receive, through the British Academy, a grant of £400 awarded by the Nuffield Foundation from a fund for the subvention of selected central publications relating to the humane sciences.

This will help the Society to meet the cost of the Sir David Ross Fascicule, and to publish as a separate paper an article by Mr. John White on *Perspective in Ancient Drawing and Painting*. This, it is hoped, will be published in the autumn of this year.

#### The Gilbert Murray 90th Birthday Fund.

To celebrate Dr. Gilbert Murray's birthday an appeal for funds was made.

The purpose of the Fund was commended to the public in a letter published in October 1955, to which the Society's President, Professor D. Tarrant, was one of the fourteen signatories. The Fund will be applied in equal proportions to the two objects to which Dr. Murray has devoted his life and genius, under a Trust deed, the main operative clause of which is as follows:—

'The Trustees of the Fund . . . declare that they hold or will hold such funds for the following purposes:—

(a) to promote the study of ancient Greek literature and thought and the propagation of Hellenic culture;

(b) to promote with the help of travelling fellowships, scholarships, grants or other means, the study of the purposes and work of the United Nations;

(c) to take such action as they may think fit for the collection of further funds to carry out the purposes set out in (a) and (b) above.'

The thirteen Trustees include the Hon. Treasurer of the Hellenic Society, Sir T. St. Quintin Hill, K.C.M.G., as well as the following members: Professor Dodds, Professor Page, and Professor Webster.

The amount of the Fund, which remains open, stands at approximately £9,000.

#### International Congresses.

Fédération Internationale des Associations d'Etudes Classiques. The Society's representative at the Meeting at Copenhagen in 1955 was Professor A. W. Gomme, F.B.A.

#### Meetings.

The following communications have been made at Meetings of the Society during the Session:—

November 11th, 1955. G. S. Kirk on 'Pherecydes of Syros'.

November 22nd, 1955 (in conjunction with the Roman Society). R. G. Goodchild on 'The New Excavations at Cyrene'.

February 17th, 1956. Professor H. Ll. Hudson-Williams on 'Plato's Phaedrus and Menexenus'.

May 4th, 1956. P. E. Corbett on 'A New Arrangement of the Sculptured Frieze from the Temple of Apollo at Bassae' (slides).

June 22nd, 1956. A. G. Woodhead on 'Cleon, Son of Cleaenetus'.

#### Provincial Meetings.

Meetings were arranged outside London in collaboration with local associations during the Session 1955-56:—

At Sheffield: J. M. Cook on 'East Aegean Model Cities' (slides).

At Exeter: Professor C. M. Robertson on 'Architecture and Sculpture and their Relation in Ancient Greece' (slides).

At Manchester: R. M. Cook on 'Ancient Sculpture and Modern Taste' (slides).

At Newcastle-upon-Tyne: Professor R. P. Winnington-Ingram on 'Phaedra in the Hippolytus'.

At Reading: A. G. Woodhead on 'The Use and Abuse of Inscriptions'.

At Northampton: Professor C. M. Robertson on 'Architecture and Sculpture and their Relation in Ancient Greece'.

At Nottingham: J. S. Morrison on 'Socrates'.

At Hull: Professor J. B. Skemp on 'Early Christian Platonism'.

At Leeds: A. G. Woodhead on 'Transport in the Ancient World'.

At Edinburgh: E. V. Rieu on 'On Translating Homer'.

At University College of North Staffordshire: Professor J. B. Skemp on 'Early Christian Platonism'.

At Southampton: Professor H. D. Kitto on 'Hamlet and Greek Tragedy'.

At Birmingham: J. S. Morrison on 'Aristophanes'.

At Northampton: Dr. F. H. Stubbings on 'Who were the Mycenaeans?'



### Administration.

The Council is grateful to Messrs. Denham, Betts and Co., who have once again acted as Honorary Auditors to the Society's Accounts.

Dr. J. P. L. Kent (Dept. of Coins and Medals, British Museum) has accepted appointment as Assistant Editor of the *Journal of Hellenic Studies*.

The ten members of Council who retire in rotation under Rule 19 are: Prof. J. M. T. Charlton, Prof. R. J. Hopper, Mr. G. S. Kirk, Mr. R. Meiggs, Prof. H. W. Parke, Prof. J. B. Skemp, Mr. J. A. Spranger, Prof. H. T. Wade-Gery, Mr. A. G. Woodhead, Prof. R. E. Wycherley.

In their place the Council have nominated the following for election: Prof. K. J. Dover, Dr. A. J. Gossage, Mr. G. T. Griffith, Mr. N. G. L. Hammond, Mr. G. T. W. Hooker, Mr. G. K. Jenkins, Mr. H. Lloyd-Jones, Mr. F. H. Sandbach, Mrs. A. D. Ure, Prof. H. D. Westlake.

### The Joint Library.

The following figures show the work of the Library during the last three years:—

	1953-54.	1954-55.	1955-56.
Books added .....	450	486	480
Books borrowed.....	4,584	4,453	4,237
Borrowers .....	611	604	619
Slides borrowed.....	4,800	4,876	4,035

It will be noted that while the number of borrowers has increased, the number of books borrowed has declined slightly. This may be accounted for by the confining to the Library of the books which now belong to the Institute of Classical Studies. Every effort is being made to provide borrowable copies of books, where their confining is observed to cause hardship. The confined Loeb, Budé, and Oxford texts have already been duplicated. Further measures being taken with a view to meeting the needs of members outside London include the compiling of a list showing where there are borrowable copies in other Libraries of confined archaeological books. Where no other copy is readily available, microfilms or photostats will be made for the use of members.

The Institute of Classical Studies added 182 books in the 'primary' category during the year, including a complete set of the Greek Commentaries on Aristotle.

The Joint Library now takes 200 periodicals, 172 by exchange. These include, in addition to British and Commonwealth publications,

periodicals from Austria (6), Belgium (7), Bulgaria (2), Czecho-Slovakia (1), Denmark (2), France (21), Germany (22), Greece (8), Holland (3), Hungary (4), Italy (23), Norway (1), Poland (3), Portugal (1), Rumania (1), Spain (6), Sweden (2), Switzerland (1), USSR (4), Yugo-Slavia (2), N. Africa (2), Argentina (1), Egypt (2), Syria (3), Turkey (1), and the U.S.A (17).

A Kodagraph MPE Microfilm Reader, which was bought by the Institute of Classical Studies, can now be made available for readers.

Much time and thought is being devoted to the planning of the new Library in Gordon Square, which will accommodate 55,000 books. Meanwhile, since the Library must remain in its present quarters for about two years, its chief problem in the immediate future is how to offer the greatest possible facilities to members without any major expenditure or reorganisation. The new steel shelves in the Seminar Room will hold about 3,000 books, and will make it possible to find shelf space for the new books coming into the Library during this interim period.

The Councils of the Hellenic and Roman Societies wish to thank Mr. R. Schwarz for the gift of thirty-eight colour slides of Greece, which have been added to the Societies' Collection. A new set of slides by Mr. P. E. Corbett, 'Greek Gods and Heroes', has also been added.

The Councils of the Hellenic and Roman Societies wish to express their thanks for gifts of books from the following: Prof. E. Akurgal, Lt.-Col. P. B. S. Andrews, Dr. A. Balil Ilianna, Mr. R. D. Barnett, Dr. E. Bielefeld, Mr. R. Browning, Mr. A. R. Burn, Mr. T. Burton Brown, Dr. A. W. Byvanck, Prof. G. Calogero, Rev. C. Creed, Mr. A. Deman, Dr. E. Dyggve, Dr. V. L. Ehrenberg, Mr. R. A. H. Farrar, Prof. L. Ferrero, Mr. S. S. Frere, Dr. D. Hamilton, Dr. J. Heller, Dr. E. Hulshoff Pol, Mr. D. K. Kanatsoulis, Dr. J. H. C. Kern, Dr. G. Klaffenbach, Prof. S. P. Kyriakides, Prof. A. Lesky, Miss E. Loeffler, Prof. W. S. Maguinness, Dr. W. Peek, Mr. E. D. Phillips, Prof. L. Robert, Prof. C. M. Robertson, Mr. A. Rowe, Dr. D. Schlumberger, Prof. P. M. Schuhl, Messrs. B. A. Seaby Ltd., Mr. H. S. Shield, Prof. O. Skutsch, Lord Strabolgi, Miss M. V. Taylor, Prof. E. G. Turner, Dr. O. Veh, Mr. C. C. Vermeule, Mr. J. B. Ward Perkins, Mr. G. Webster, Prof. T. B. L. Webster, the British School at Athens, the British School at Rome, Northampton Public Libraries.



# THE SOCIETY FOR THE PROMOTION OF HELLENIC STUDIES

## BALANCE SHEET AT DECEMBER 31, 1955.

1954.	Liabilities.	£	s.	d.	£	s.	d.	Assets.	£	s.	d.
384	Debts Payable .....				587	8	6	Cash in Hand—			
167	Subscriptions Paid in Advance .....				221	17	0	At Bank—Current Accounts .....	623	16	0
2,949	Endowment Fund .....				2,949	12	0	At Bank—Deposit Account .....	900	0	0
2,481	Life Compositions—							Petty Cash .....	9	5	4
	At January 1, 1955 .....	2,481	4	0							
	Received during the year .....	84	0	0				Debts Receivable .....	1,533	1	4
								Investments at Cost (approximate Market Value at December 31, 1955, £4,198) .....	1,066	1	7
	Less carried to Income and Expenditure Account—	2,565	4	0				Stock of Journals and Paper and Publications .....	4,510	0	0
	Deceased Members .....	31	10	0				Sundry Debit Balances—	480	0	0
								Slides Account .....			
	Publications Fund .....				2,533	14	0	Joint Library Books Account .....	17	4	4
	Sundry Credit Balances—				250	0	0				
	Joint Library Books Account .....										
10	Slides Account .....				11	7	7				
738	Surplus Account—										
	At January 1, 1955 .....	737	12	2							
	Add Excess of Income over Expenditure for the year ended December 31, 1955 .....	334	16	0							
					1,072	8	2				
					£7,626	7	3				
					£6,729						

The Society's share of the capital value of the Libraries and Photographic Department is not included as an Asset in the above Balance Sheet.

We have audited the Books of The Society for the Promotion of Hellenic Studies for the year ended December 31, 1955, and have received all the information and explanations we have required, and in our opinion the above Balance Sheet gives a true and correct view of the Society's financial position at December 31, 1955, according to the Books of the Society and the information and explanations furnished to us.

HOLBORN HALL,  
GRAY'S INN ROAD,  
LONDON, W.C.1.  
February 1, 1956.

DENHAM, BETTS & CO., *Certified Accountants.*

## INCOME AND EXPENDITURE ACCOUNT FOR THE YEAR ENDED DECEMBER 31, 1955.

1954.	Expenditure.	£	s.	d.	1954.	Income.	£	s.	d.
430	To Salaries and State Insurance .....	435	15	7	623	By Subscriptions Received .....	2,714	0	8
139	" Printing and Stationery .....	105	15	10	225	" Income Tax Recoverable (Deeds of Covenant) .....	206	12	8
108	" Postages .....	106	1	11	47	" Life Compositions (Deceased Members) brought into Revenue .....	31	10	0
75	" Sundry Expenses .....	134	10	3	132	" Dividends .....	168	11	4
868	Balance from 'Journal of Hellenic Studies' Account .....	967	13	3	—	" Interest on Deposit Account .....	30	7	8
100	" Contribution to Joint Sinking Fund .....	100	0	0	213	" Miscellaneous Receipts .....	50	2	7
568	" Share of Library Maintenance Account .....	666	12	1					
100	" Grant for Books .....	100	0	0					
—	" Contribution to Publications Fund .....	280	0	0					
890	" Balance being Excess of Income over Expenditure .....	334	16	0					
		£3,201	4	11			£3,201	4	11
		£3,278							

1954.	Expenditure.	£	s.	d.	1954.	Income.	£	s.	d.
1,526	To Cost of Vol. LXXV—				623	By Sales including back Volumes .....	596	11	1
141	Printing and Paper .....	1,586	7	9	33	" Receipts from Advertisements .....	59	1	3
65	Drawings and Engraving .....	210	18	9	250	" Grant from Jowett Trust .....	250	0	0
112	Editing and Reviews .....	119	3	9	50	" Contribution from London University .....	50	0	0
	Postage and Packing, etc. ....	115	7	5	—	" British School at Athens .....	65	15	4
					—	" Reading University .....	30	0	0
					—	" Mrs. Bryher .....	25	0	0
	" Purchases of back Volumes .....	2,031	17	8	868	" Balance to Income and Expenditure Account .....	967	3	3
		12	3	3					
		£2,044	0	11			£2,044	0	11
		£1,844							



## THE JOINT LIBRARY OF THE HELLENIC AND ROMAN SOCIETIES

## LIBRARY MAINTENANCE ACCOUNT FOR THE YEAR ENDED DECEMBER 31, 1955.

[illegible]

## JOINT LIBRARY BOOKS ACCOUNT FOR THE YEAR ENDED DECEMBER 31, 1955.

1954.		1954.		1954.	
	£	s.	d.		£
To Balance at December 31, 1954, brought forward	7	11	2	By Balance brought forward	131
" Purchases	23	5	3	" Receipts from Sales of Catalogues, Duplicate Books, etc.	100
" Binding	78	10	9	" Grants—Hellenic Society	100
" Balance carried forward	11	7	7	" —Roman Society	200
				" Balance carried forward	—
					£331
					£346

LANTERN SLIDES AND PHOTOGRAPHS ACCOUNT FOR THE YEAR ENDED DECEMBER 31, 1955.

	£	s.	d.
To Purchases .....	72	18	5
33 Salaries .....	80	0	0
39 " .....	10	0	0
43 Acquisitions List .....	10	13	7
" Advertising .....	3	3	0
10 " Balance carried forward .....	—	—	—
	£166	15	0
£115	£115		

THE SINKING FUND (JOINT HELLENIC AND ROMAN SOCIETIES) FOR THE YEAR ENDED DECEMBER 31, 1955.

To Amount expended during the year—	£	s.	d.
Part of Dilapidations on renewal of Lease .....	318	12	10
Fee of University Surveyor .....	15	15	6
Gas Cooker for Basement Flat .....	14	10	6
Share of Cost of exterior painting .....	118	0	6
Balance of Fund at December 31, 1955, with the London Trustees .....	550	14	10
Savings Bank .....			
	<u>£1,017</u>	<u>14</u>	<u>2</u>



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Studies vol LXV,

1956

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